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THE RELATIVE RELIABILITY OF WORDS AND NONSENSE SYLLABLES AS LEARNING MATERIAL

BY FRANCES COON DAVIS

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Most of the experimental work in memorization has been done with nonsense syllables. There have been objections to nonsense syllables because they are so difficult and uninteresting that subjects dislike to learn them and because the results so obtained are not applicable to every day experiences. The answers to these objections have been the time honored propositions of Ebbinghaus that nonsense syllables are more homogeneous than other types of learning material; they are numerous and capable of quantitative variation. The two latter statements seem to be equally true for words.

The assumption of greater homogeneity of nonsense syllables has been further amplified to the now generally accepted conclusion that that type of material is more reliable than words inasmuch as the learning scores obtained with nonsense syllables vary less from list to list and from individual to individual than do those similarly obtained with words. Presumably lists of nonsense syllables are all equally unfamiliar to all individuals; therefore any variation in learning scores would be less than that which obtained for lists of words.

However, it does not seem entirely unreasonable to require some evidence to substantiate such an assumption. The specific purpose of the present experiment is to determine whether the number of trials required for learning a list of

nonsense syllables does vary less from list to list than does the number of trials required to learn a comparable list of words.

Procedure

Six subjects each learned twenty lists of twelve nonsense syllables and twenty lists of twelve monosyllabic words.

The words used were selected in the following manner: Out of all the possible combinations of a vowel between two consonants those which occur in Webster's Collegiate Dictionary without the denominations nautical, mechanical, foreign, obsolete, colloquial, or the like were accepted as the words to be learned. Of these words all those not in common use were discarded. The object was to secure words which might reasonably be assumed to have meaning for all the subjects.

Out of the words so qualified twenty-one lists of twelve items each were chosen by chance. The position of the items within their respective lists was also determined by chance, subject to the following arbitrary rules: (1) No two items with the same initial consonant occur in the same list. (2) No two items with the last two letters the same occur in the same list. This condition was to avoid rime. (3) No two items with the same vowel or the same final consonant occur following each other in a list.

In choosing the nonsense syllables an effort was made to secure those syllables out of all possible combinations which would have the least meaning for the subjects and so presumably the greatest homogeneity. To this end the syllables which Glaze [1] evaluated as having an association value of 20 percent or less were chosen as the nonsense material. All the syllables in his lists which contained the letter combinations XY or YZ were omitted; it was assumed that these letters occurring in the same order as their position in the alphabet might prejudice learning in their favor.

Out of the syllables so selected twenty-one lists of twelve syllables each were chosen by chance. Their arrangement in

[1] Glaze, J. A., The Association Value of Nonsense Syllables, *J. Genetic Psychol.*, 1928, 35, 255–269.

lists was by chance and subject to the same arbitrary rules as in the case of the words.

Learning was by the anticipation method with a Chicago memory drum operated by hand at the rate of one item per three seconds. The method was explained to the subject on the first day of the experiment; after that the learning proceeded without further instructions except that each day the experimenter mentioned the nature of the day's learning material whether nonsense or words. Even this precaution was soon unnecessary.

Nonsense syllables were learned the first day of the experiment and every other day thereafter; on the alternate days lists of words were learned. This alternation of the two materials throughout the experiment should have made practice about equal for the two conditions. The first two days of the experiment served to familiarize both experimenter and subject with the whole experimental situation; the records obtained during that time are not included in any of the calculations.

In his anticipations the subject spelled the nonsense syllables and pronounced the words. This procedure was adopted because hard syllables such as here used are practically incapable of any uniform pronunciation, and therefore some arbitrary standard of pronunciation would have been necessary. On the other hand spelling the words would have destroyed their character as words.

One repetition of the list followed directly after the preceding one until the subject had anticipated all the items correctly once. Each list was preceded by its number in the series from I to XLII, and after the first repetition the subject was expected to give the initial item of the list when this number appeared in the aperture of the drum. In this way the subject learned all twelve items in each list.

The six subjects were all graduate students in psychology. All were experienced experimental subjects and had had some previous acquaintance with the method of learning employed. All were interested in experimental work and were consequently most coöperative and faithful. In fact, the subjects

were probably as reliable if not more so than could reasonably be hoped for in any experiment with human beings.

The learning took place every morning from Monday to Friday inclusive during five successive weeks; before the final week of the experiment there was an interval of one week during which time it was necessary for one of the subjects to be absent from the city.

The learning always took place between 8 A.M. and noon in the same room with the same experimenter. Subject Sh is the only exception to this condition; several times it was necessary for him to learn in the afternoon and once in the evening; on other occasions he learned just before class or when he should have been in class. Most of the inconsistencies of the data occur in his records as will be pointed out later.

The subject was seated facing the memory drum with a south light coming in over his shoulder. The experimenter sat on the left of the subject and the drum so as to operate the drum conveniently with her left hand. In this way her right hand was free to record the subject's responses. A book open at a ninety degree angle formed a convenient shield between the record sheet and the subject; it was high enough to prevent the subject's eyes from straying toward the experimenter's writing hand and yet low enough so as not to obstruct the experimenter's view of the drum.

The subject's responses were recorded on a previously prepared record sheet.

Results

Individual Variability From List To List

The averages of the twenty learning scores for each individual and for each type of material are presented in columns two and five of Table 1. The nonsense syllables give uniformly and pronouncedly higher scores than do the words; there is no overlapping.

In the third and sixth columns is given the standard deviation for each average. As might be expected the higher means for nonsense syllables have the higher standard deviations; that is, in terms of prediction, if the experiment

were repeated the mean of the word lists for each individual would probably be nearer its obtained mean than would the mean of the nonsense syllable lists. It is possible to predict

TABLE 1

	Nonsense Syllables			Words		
Individual	M	σ	$\frac{100\sigma}{M}$	M	σ	$\frac{100\sigma}{M}$
M	10.6	3.09	29.14	3.85	1.28	33.14
St	10.4	3.89	37.41	4.8	1.25	26.02
Si	9.4	3.8	40.43	4.35	0.61	14.11
L	8.95	5.64	63.05	2.05	0.86	42.2
Sa	9.00	4.84	53.74	3.05	0.77	25.34
Sh	10.45	3.56	34.03	4.5	1.4	31.02
Group	9.8	4.302	43.89	3.74	1.497	40.02

the true mean within narrower limits above and below the obtained mean in the case of words than in the case of nonsense syllables.

In scientific work, however, we are primarily concerned with the reliability of the difference between two means and with the possibility of predicting whether the same kind of difference will be obtained if the experiment is repeated. For this purpose, the relative variability of the mean, that is, the ratio of the mean to the standard deviation, is the really significant criterion.

In the treatment of the present results the coefficient of variability $\frac{100\sigma}{M}$ has been used as such a measure of relative variability. These values are given in the fourth and seventh columns of Table 1 for nonsense syllables and words respectively.

With only one exception, subject M, relative variability like absolute variability is greater for nonsense syllables. In the case of this one exception the difference is not very great; the fact that this individual who made the highest average in the group for nonsense syllables ranked fourth from the top in the word scores encourages the belief that this anomaly is the result of some individual peculiarity. In any case five out of the six subjects do show greater variability for words.

From these data we are forced to conclude that it is the nonsense syllables which exhibit the greater list to list variability when this variability is measured in either absolute or relative terms. For these experimental conditions it is evident that Ebbinghaus' assumption does not hold; nonsense syllables are less homogeneous than words.

Group Variability

Thus far each individual has been considered separately and has been compared only with himself; in the last line of Table 1 are given the same data for the group; these standard deviations and the coefficients of variability are not the averages of the individual data, but they have been calculated for the group as a whole.

Absolute variability is considerably greater for the nonsense material; in other words prediction of group averages is more uncertain for that material.

Relative variability, however, is only slightly greater for the nonsense material; the difference is statistically almost insignificant—not at all the result that might have been expected from the fact that five of the six subjects exhibited a much greater relative variability for words.

This seeming inconsistency in the results may be explained easily enough by calling attention to the fact that variation from individual to individual is a factor in the group results as well as is variation from list to list. As will be shown later there is some evidence to indicate that relative variability from individual to individual is greater for words than for nonsense syllables. Such a condition would tend to counterbalance the greater list to list variability of nonsense syllables and produce the rather negligible difference just pointed out.

It is true then that for a small group absolute variability is greater for nonsense material than for words and that relative variability is probably about the same for both types of material.

Reliability Of Individual Differences

Memorization experiments are usually concerned with the establishment of a statistically reliable difference between

two means, and all other factors being constant it may be assumed that the greatest reliability between two means can be secured with the least variable material. The phenomenon of variability is of significance then only in relation to its function in determining the reliability of the difference between two means.

Our experiment was designed to test one aspect of this assumption. Since words vary less from list to list for individuals, can the greater reliability between two individual means be secured with that type of material? Will words or nonsense syllables give greater reliability of the difference between individual means?

TABLE 2

	Nonsense Syllables			Words		
Individuals	D	σ_d	$\frac{D}{\sigma_d}$	$\frac{D}{\sigma_d}$	σ_d	D
M–St	0.20	1.111	0.180	2.380	0.399	0.95
M–Si	1.20	1.095	1.096	1.581	0.316	0.50
M–L	1.65	1.439	1.147	5.222	0.345	1.80
M–Sa	1.60	1.288	1.242	2.398	0.334	0.80
M–Sh	0.15	1.054	0.142	1.537	0.423	0.65
St–Si	1.00	1.217	0.822	1.449	0.311	0.45
St–L	1.45	1.533	0.946	8.095	0.339	2.75
St–Sa	1.40	1.388	1.070	5.327	0.329	1.75
St–Sh	0.05	1.157	0.043	0.716	0.419	0.30
Si–L	0.45	1.522	0.296	9.721	0.237	2.30
Si–Sa	0.40	1.376	0.291	5.904	0.220	1.30
Si–Sh	1.05	1.164	0.902	0.440	0.341	0.15
L–Sa	0.05	1.662	0.030	3.855	0.259	1.00
L–Sh	1.50	1.492	1.005	6.670	0.367	2.45
Sa–Sh	1.45	1.343	1.080	4.063	0.357	1.45

Our experiment permits us to compare the relative reliability of 15 pairs of individual means for the two types of material. The obtained differences and their obtained reliabilities are given in Table 2. In calculating the reliabilities of the differences their standard errors were obtained by the formula $\sigma_d = \sqrt{\sigma_1^2 + \sigma_2^2}$ and the σ_d's so obtained were divided into the obtained differences. These values, $\frac{D}{\sigma_d}$, are used throughout as a measure of reliability. They may be translated into chances if such a procedure seems desirable; but there is no doubt that greater numerical accuracy is secured in the values themselves. As a basis of

comparison it may be stated that a value of 3 or greater is usually accepted as indicative of perfect reliability.

Comparing the reliabilities of the differences obtained for words with those for nonsense syllables the uniformly greater reliability of the word differences is at once apparent. There is only one exception in the total of 15 comparisons. In this instance (Si–Sh) the fact that both reliabilities are quite low and that the conditions of learning for the individual Sh were more variable than were those of the other subjects would lead one to refrain from attaching too great significance to this exception.

Not only are the word differences higher in practically every case, but the absolute amounts of difference between the reliabilities of words and those of nonsense syllables are so striking as to preclude any possibility of chance fluctuations. If all the nonsense reliabilities are arranged in order from lowest to highest and all the word reliabilities are arranged in the same manner, only two of the word reliabilities will fall as low as the highest nonsense reliability. Both of these exceptions involve subject Sh whose exception to the learning conditions has already been noted.

These comparisons demonstrate beyond any reasonable doubt that on the whole the individual differences obtained with lists of words are more reliable than those obtained with the same number of lists of nonsense syllables. It is safer to predict a true individual difference from an obtained individual difference when that difference has been obtained with words than when it has been obtained with an equal number of lists of nonsense syllables.

If a more reliable difference can be obtained with a given number of lists of words, then a difference of a certain desired degree of reliability can be obtained with fewer lists of words than of syllables. Words are thus the more economical material to use for individual comparisons, inasmuch as fewer records are required in order to obtain a given degree of reliability.

Relation Of Variability To Practice

In all list to list comparisons practice is necessarily involved, and it is entirely possible that the greater variability of nonsense syllables from list to list is occasioned in part by the fact that this material is more subject to the effects of practice.

TABLE 3

First Ten Lists

Individual	Nonsense Syllables M	σ	$\frac{100\sigma}{M}$	Words M	σ	$\frac{100\sigma}{M}$
M	12.3	3.7	30.08	4.6	1.09	23.68
St	12.2	2.8	22.95	5.2	1.26	24.23
Si	11.7	3.6	30.77	4.8	1.09	22.92
L	12.2	5.4	44.26	2.1	0.50	23.81
Sa	11.8	5.5	46.61	3.3	0.77	23.33
Sh	12.3	4.2	33.74	5.2	1.09	20.96
Group	12.1	4.2	34.64	4.2	1.50	35.95
Last Ten Lists						
M	8.9	1.22	13.71	3.1	0.95	30.65
St	8.1	2.02	27.41	4.4	1.09	24.77
Si	7.2	2.07	28.75	3.6	1.18	32.77
L	5.7	1.67	29.29	2.0	1.09	54.50
Sa	6.1	1.30	21.31	2.8	0.71	25.36
Sh	8.6	1.14	13.24	3.8	1.34	40.53
Group	7.4	2.1	28.38	3.3	1.30	39.82

Practice. As might be expected all subjects show improvement in both types of material (Table 3). The group exhibits the greater practice effect in both absolute and relative terms for the nonsense material; with nonsense syllables the improvement from the first ten lists to the second ten lists is 4.7 repetitions or 38 percent; with words the improvement is 0.9 repetitions or 21 percent. The individual records show the greater absolute improvement with the nonsense material in all cases; five out of the six cases show greater relative improvement as well.

It seems then to be true that the greater variability of the nonsense material is due in part to the fact that it is more subject to the effects of practice than is the sense material.

Variability in ten lists. All the conclusions thus far stated have been based on the data from twenty lists of nonsense

syllables and twenty lists of words. It is pertinent to inquire what the results would be if the comparison were made on the basis of only ten lists of each material. The data for this comparison are given in Table 3.

Without exception the absolute individual variability from list to list is greater for nonsense syllables; in five of the six cases relative variability is greater for that type of material.

For the group absolute variability is greater with nonsense material; there is no appreciable difference between the two materials with respect to relative variability.

The results obtained with ten lists are thus the same as those obtained with twenty lists; the experiment might have been continued only half as long and the results would have been entirely comparable.

Variability for ten lists after practice. In the latter part of Table 3 are presented the results considering only the last ten lists—after the subjects had already learned eleven lists of each type of material.

Absolute variability for both individuals and group is greater with nonsense syllables; but relative variability for five of the six individuals and for the group as a whole is greater for words.

In opposition to the results for ten and twenty lists when the subjects are not thoroughly practiced, these data indicate that for practiced subjects words are relatively more variable from list to list than are nonsense syllables.

It seems that for practiced subjects there is some justification for Ebbinghaus' assumption of greater homogeneity of nonsense syllables. Both individual and group results confirm this statement.

Variability From Individual To Individual

We have noted that the nonsense syllables are relatively the more variable from list to list for the majority of the individuals, while the two materials are practically on a par in respect to group variability. The group results are obviously influenced by the variability from individual to

individual as well as by the list to list variability. It has been suggested that the discrepancy between the individual and the group results can be explained on the assumption that it is the words that exhibit the greater variability from individual to individual.

Our data furnish some evidence in support of this assumption, although our experiment was not designed to study this factor of individual to individual variability. No very confident generalization can be made on the basis of six individuals.

TABLE 4

List	Nonsense Syllables			Words		
	Average	A.D.	$\frac{\text{A.D.}}{\text{A}}$	Average	A.D.	$\frac{\text{A.D.}}{\text{A}}$
1	21.17	2.39	0.11	4.17	1.22	0.29
2	14.67	1.88	0.13	4.00	1.00	0.25
3	14.00	3.00	0.21	4.67	1.33	0.29
4	10.67	1.44	0.13	4.50	1.67	0.37
5	9.17	1.83	0.20	4.17	1.11	0.27
6	12.83	2.77	0.22	4.17	0.83	0.20
7	12.17	3.88	0.32	4.50	1.83	0.40
8	9.17	1.17	0.12	4.00	1.00	0.25
9	9.17	1.50	0.16	4.17	1.22	0.29
10	8.83	2.22	0.25	3.67	1.00	0.27
11	7.33	1.67	0.23	3.17	0.55	0.17
12	6.17	1.50	0.24	4.33	0.67	0.15
13	7.50	1.17	0.16	3.50	0.83	0.23
14	7.50	2.83	0.38	4.00	1.00	0.25
15	7.00	1.50	0.21	2.83	1.36	0.48
16	8.67	2.00	0.23	2.67	0.44	0.16
17	5.83	0.88	0.15	3.17	1.22	0.38
18	7.33	1.33	0.18	2.17	0.28	0.13
19	7.67	2.33	0.30	3.83	1.39	0.34
20	8.67	1.55	0.18	3.17	1.50	0.48

The average, the average deviation, and the ratio of the average deviation to the average for each of the lists of both types of material are given in Table 4. The average deviation has been used as the measure of absolute variability, and the ratio of this value to the mean as a measure of relative variability. The use of the standard deviation on the basis of six records is hardly justified.

In Table 5 these measures of absolute variability have been arranged in two frequency distributions—one for each type of material. The nonsense syllables exhibit the wider range of variability and a general trend toward the higher values.

TABLE 5

A.D.	Frequencies	
	Nonsense Syllables	Words
0– .49		2
.5– .99	1	4
1.0–1.49	4	11
1.5–1.99	7	3
2.0–2.49	4	
2.5–2.99	2	
3.0–3.49	1	
3.5–3.99	1	

The measures of relative variability have been similarly arranged in Table 6.

TABLE 6

$\frac{\text{A.D.}}{\text{A}}$	Frequencies	
	Nonsense Syllables	Words
.10–.14	4	1
.15–.19	5	3
.20–.24	7	2
.25–.29	1	8
.30–.34	2	1
.35–.39	1	2
.40–.44		1
.45–.49		2

It is the words that exhibit the wider range of relative variability and a general trend toward the higher values.

Our meagre data thus indicate that absolute variability from individual to individual is greater for nonsense syllables, while the relative variability from individual to individual is greater for words. This conclusion is also indirectly supported by the apparent discrepancy between the individual and the group results for list to list variability.

Summary

I. *Absolute variability.* Nonsense syllables invariably exhibit the greater absolute variability. They vary more from individual to individual, and from list to list both before and after practice for both the individuals and the group. On the basis of a given number of records one can predict the mean less accurately with nonsense syllables than with words.

This fact is not of much significance, however, from the standpoint of the prediction of a difference between two means.

II. *Relative variability without previous practice.*

1. Nonsense syllables exhibit the greater relative varia-

bility from list to list for five of the six subjects for both ten and twenty lists.

2. As might be expected, the differences between the individual means are less reliable for the nonsense syllables than for the words. In order to obtain a desired reliability for the difference between individual means, a larger number of lists must be employed in the case of nonsense syllables than in the case of words.

3. Nonsense syllables are less variable from individual to individual, although the data are too few for any very confident generalization.

4. The two types of material are on a par in respect to group variability for both ten and twenty lists. Evidently this fact is to be explained on the assumption that the greater list to list variability of nonsense syllables is counterbalanced by their lesser variability from individual to individual.

III. *Relative variability after practice.* When the subjects are thoroughly practiced, nonsense syllables are relatively less variable from list to list for five of the six individuals and for the group as a whole. This statement is based on the data for the last ten lists after the subjects had memorized eleven lists of each material.

Only two of these facts support the Ebbinghaus doctrine of the greater homogeneity of nonsense syllables: (1) Nonsense syllables exhibit less relative variability from individual to individual, and (2) after considerable practice they exhibit less relative variability from list to list. The two types of material are on a par with respect to group variability with unpracticed subjects, and in all other respects lists of words, contrary to the contention of Ebbinghaus, are the more homogeneous material.

Ebbinghaus assumed that the more homogeneous, that is, the less variable, material should be selected for memory experiments. We wish to point out that the only reason for choosing the more homogeneous type of material is the practical one of economy of effort. As we have previously indicated, fewer scores with the less variable material are required in order to secure the desired degree of reliability

for the difference between two means; that is, the difference can be reliably established on the basis of fewer subjects or fewer lists with the more homogeneous material.

However, even though it were granted that nonsense syllables were the more homogeneous and that with this material fewer records were required for the purposes of the experiment, yet we wish to call attention to the further fact that this assumed advantage would be far outweighed by the fact that words are the easier material to memorize and hence that a much larger number of records can be secured with this type of material in the same amount of time.

Thus the assumption of Ebbinghaus is justified neither in fact nor in theory.

(Manuscript received July 1, 1929)

THE RELATIVE VARIABILITY OF NONSENSE SYLLABLES AND WORDS

BY FLORENCE M. SAUER

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The Ebbinghaus dictum of the greater homogeneity of nonsense syllables assumes that the learning scores of nonsense syllables are less variable than those of words. This assumption involves two separate questions:

(1) The relative variability of the two materials from list to list.

(2) The relative variability from individual to individual.

Miss Davis investigated the first aspect of the problem and found that the coefficient of the list to list variability was less for words than for syllables, and the difference between individual means for words was more reliable than for syllables. It was found that practice decreased these coefficients more for syllables than for words and as a consequence the coefficients for the last ten lists were smaller for the nonsense material.

This experiment was designed to test the second aspect of the Ebbinghaus doctrine, namely, the relative variability of the two materials from individual to individual. Miss Davis found that the lists of words exhibited the higher individual variability with five subjects. Our experiment was designed to test this conclusion on the basis of a larger group of subjects. We secured records from twenty subjects for five lists of each of the two materials.

lower See page 225

Our subjects consisted of a relatively heterogeneous group: fifteen undergraduates from the introductory classes of psychology, and five graduate students of psychology.

The learning materials employed in this experiment were five lists of each material. Each list consisted of twenty-four items. The lists of nonsense syllables and words were of the same length. The nonsense syllables were arranged in the

list according to chance, subject to the usual precautions. (1) All syllables suggesting a meaning were eliminated. (2) No two items with the same initial consonant followed each other in the same list. (3) No two items with the same vowel or final consonant followed each other in the same list. The words selected were those of common usage. They were monosyllables consisting of two consonants with a vowel in the middle. They were arranged in the list according to chance, and words similar in spelling or meaning were never presented contiguously.

The lists were presented serially with a memory drum. Each item was exposed for three seconds and they were learned to the point where the subject was able to anticipate the coming item for two successive trials. The subject anticipated each syllable by spelling it aloud within the exposure of the syllable preceding, while in the case of words, the subject merely pronounced it.

We secured two sets of data: first, the variability of the two materials from individual to individual in respect to the lists as a whole; and second, the individual variability in respect to each item of the list.

In the first case, we computed the mean number of trials to learn each list, the standard error of its distribution, and its coefficient of variability. These data for each of the lists for both materials are given in Table I. For example, a group of twenty subjects required an average of 7.35 trials to memorize the first list of words, and the standard deviation of these twenty records is 2.90. A coefficient of variability represents the ratio of the standard deviation to the average. The corresponding coefficient of variability is 3.95.

In order to obtain the individual variability of the separate items, we recorded the temporal order in which the twenty-four items were learned by each individual. For example, any item of the list might be learned first by one individual, and be the fifth to be memorized by another individual, or it may be the tenth in order of mastery by some other individual.

We then computed the means and the standard deviations

of these individual rank orders, and the corresponding coefficients of variability for each item of each list. In case two or more items were learned on the same trial, each was given the mean value of the ordinal positions represented. The means for each item of each list for the words and syllables are given in Tables II and III respectively. The

TABLE I

INDIVIDUAL VARIABILITY OF LISTS

Words				Syllables			
Lists	Average	S.D.	Coefficient of Variability	Lists	Average	S.D.	Coefficient of Variability
I	7.25	2.90	3.95	II	18.85	5.08	2.69
III	7.00	2.97	4.24	IV	16.65	3.71	2.23
V	4.60	1.66	3.46	VI	13.85	3.34	2.41
VII	5.10	2.26	4.43	VIII	11.05	2.92	2.64
IX	4.40	1.83	4.16	X	9.85	1.85	1.88

TABLE II

MEANS FOR EACH LIST OF WORDS

Number of the Item	Lists I	III	V	VII	IX	Average
1	6.53	4.93	6.78	7.20	6.60	6.41
2	6.50	9.25	6.50	7.63	7.58	7.49
3	9.64	9.23	7.80	8.60	11.23	9.30
4	12.93	14.13	12.70	10.93	11.88	12.51
5	12.48	11.58	7.68	10.10	7.78	10.12
6	13.73	12.05	9.85	13.85	10.88	12.07
7	14.49	12.20	11.88	8.48	11.78	11.77
8	12.65	14.95	14.95	12.58	8.88	12.80
9	9.70	11.80	13.65	14.68	16.68	13.30
10	14.68	10.53	11.23	14.48	11.58	12.50
11	18.88	17.48	11.93	12.45	13.23	14.89
12	13.95	13.60	10.03	12.15	12.35	12.42
13	13.52	12.65	15.78	17.68	13.88	14.70
14	14.64	17.58	17.90	14.98	14.08	15.84
15	11.58	13.63	14.90	17.20	10.45	13.55
16	12.92	15.73	19.10	13.83	15.23	15.36
17	14.30	15.10	15.28	12.55	16.08	14.66
18	12.54	14.95	15.48	15.13	13.70	14.36
19	12.18	16.33	16.55	15.78	15.53	15.27
20	12.03	12.45	12.68	15.95	14.25	13.47
21	12.03	7.40	8.95	10.58	17.55	11.30
22	11.45	10.45	13.35	10.15	13.53	11.77
23	14.53	11.65	11.60	10.78	11.50	12.01
24	11.23	10.40	13.50	12.90	12.85	12.18

standard deviations of these measures are given in Tables IV and V respectively, and the corresponding coefficients of variability are listed in Tables VII and VIII respectively.

For example, the average rank order in which item number one of the first list of words was learned by the twenty individuals is 6.53 (Table II), the standard deviation of the distribution of the twenty ranks for this item is 4.52 (Table IV), and its coefficient of variability is 69.22 (Table VII).

TABLE III

MEANS FOR EACH LIST OF SYLLABLES

Number of the Item	Lists II	IV	VI	VIII	X	Average
1	1.67	3.38	2.80	3.58	2.53	2.79
2	4.33	5.83	2.80	3.90	3.90	4.19
3	3.88	7.00	7.40	8.35	8.10	6.95
4	9.08	9.23	7.13	11.53	8.88	9.17
5	8.48	10.95	10.13	9.85	10.60	10.00
6	13.15	12.05	15.48	9.18	13.90	12.75
7	13.08	13.45	14.75	7.25	16.98	13.10
8	10.60	17.08	15.65	16.60	13.68	14.72
9	11.35	16.90	13.50	13.65	14.30	13.94
10	15.05	16.55	15.53	13.85	16.03	15.40
11	13.75	13.40	12.80	13.05	12.10	13.02
12	12.78	16.90	17.00	17.13	18.05	16.37
13	16.08	15.95	13.53	20.93	17.13	16.22
14	16.23	12.38	17.83	16.38	14.75	15.51
15	13.58	19.03	14.00	14.70	13.48	14.96
16	17.93	15.58	11.85	14.43	12.75	14.51
17	15.05	18.25	13.88	11.60	18.48	15.45
18	18.35	18.28	11.55	18.30	14.85	16.27
19	18.33	18.40	15.13	17.60	10.78	16.05
20	17.45	11.78	13.70	16.08	11.53	14.11
21	10.88	10.08	18.45	11.03	12.40	12.57
22	16.20	6.83	17.80	11.73	12.20	12.95
23	14.40	6.23	11.98	12.15	12.40	11.43
24	7.50	4.55	5.88	7.15	9.75	6.97

(1) INDIVIDUAL VARIABILITY OF LISTS

In absolute terms (standard deviation records) the lists of syllables tend to exhibit the greater variability. This fact might have been predicted from their larger learning scores.

In relative terms (coefficient of variability) the lists of words are the more variable material. The differences are quite pronounced. All the coefficients for the word lists are considerably larger than any of those for the syllables. This result confirms the findings of Miss Davis' experiment.

It may be noted from the table that practice decreases the mean scores for both words and syllables and the standard deviations for both words and syllables, but there is no

evidence that this amount of practice tends to decrease the coefficients of variability.

(2) Individual Variability of Items

The standard deviations of the various items are listed in Tables IV and V and for the purpose of comparison, the frequency distributions of these values for the two materials are given in Table VI.

TABLE IV

Individual Variability of Items

Number of the Item	Words Lists I	III	V	VII	IX	Average
1	4.52	1.91	5.21	2.29	3.60	3.51
2	4.43	5.06	6.53	3.83	3.62	4.69
3	4.95	4.82	4.71	4.63	4.67	4.76
4	6.86	6.98	5.87	5.55	5.12	6.08
5	6.63	7.03	3.28	4.87	4.54	5.22
6	6.91	5.06	4.28	6.05	5.43	5.55
7	5.91	5.59	6.25	4.24	4.36	5.27
8	6.49	5.56	4.17	5.97	6.31	5.70
9	4.85	5.81	5.78	5.55	6.17	5.63
10	7.43	5.34	4.51	6.43	5.88	5.92
11	4.00	6.30	6.45	5.29	7.00	5.81
12	2.30	6.71	4.86	6.10	6.66	5.33
13	6.37	6.50	5.84	5.61	5.76	6.01
14	6.99	6.32	6.08	5.62	5.71	6.14
15	7.09	6.48	5.94	6.63	4.99	6.23
16	6.91	7.50	5.22	6.00	5.87	6.30
17	6.00	7.01	6.69	6.78	5.55	6.21
18	6.56	6.61	5.60	6.43	6.08	6.26
19	5.97	5.51	6.40	6.22	5.73	5.97
20	5.33	5.60	5.71	5.69	5.45	5.56
21	5.59	5.32	5.19	4.12	4.68	4.98
22	5.18	5.14	6.11	5.11	6.77	5.66
23	4.74	4.92	4.55	4.92	5.72	4.97
24	4.56	4.18	6.29	4.59	6.37	5.20
Total	136.30	137.26	130.52	128.52	132.04	
Grand Total	664.64					

It may be noted that the grand total or the sum of all the deviations for the words is slightly larger than for the syllables. The two values are 664.64 and 608.96. From the frequency distribution table it may be noted that the syllables have more of the smaller values and fewer of the larger values. The differences between the two sets of values are not large, and the reliability of the difference may be open to question.

In as much as the means of Tables II and III are based

upon the rank order in which the items are learned, and the sums of these values for any list are approximately identical for the various lists irrespective of the material, it follows

TABLE V

INDIVIDUAL VARIABILITY OF ITEMS

Number of the Item	Syllables Lists II	IV	VI	VIII	X	Average
1	1.10	2.56	5.11	3.07	1.67	2.70
2	2.98	4.25	4.83	2.91	2.44	3.48
3	2.31	4.90	3.47	5.40	5.42	4.30
4	4.38	5.18	4.15	5.97	5.98	5.13
5	5.58	5.61	4.98	5.15	2.36	4.74
6	5.35	6.54	5.95	5.21	6.00	5.81
7	5.34	4.51	5.37	5.51	5.88	5.32
8	6.27	5.28	4.31	5.32	5.93	5.42
9	4.92	4.83	5.73	5.72	2.57	4.75
10	5.47	4.68	6.67	5.67	4.79	5.46
11	5.61	5.74	6.34	4.97	4.68	5.47
12	5.11	5.11	6.50	6.36	4.12	5.44
13	6.33	5.81	5.61	2.62	5.27	5.13
14	6.19	5.96	5.07	7.40	7.56	6.44
15	6.53	4.80	6.87	5.79	6.80	6.16
16	6.56	5.57	6.13	5.55	6.02	5.97
17	6.01	5.35	6.98	5.95	5.27	5.91
18	5.69	4.51	6.47	3.71	6.33	5.34
19	3.91	3.91	6.87	5.46	5.37	5.10
20	6.35	5.25	5.23	5.58	4.78	5.44
21	5.39	5.05	4.88	5.47	5.89	5.34
22	4.55	4.48	3.60	6.38	6.59	5.12
23	3.73	1.94	5.63	4.76	5.60	4.33
24	3.14	2.39	2.58	3.91	5.46	3.50
Total	118.80	114.21	129.33	123.84	122.78	
Grand Total	608.96					

TABLE VI

DISTRIBUTION TABLE OF INDIVIDUAL VARIABILITY OF ITEMS

S.D.	Words	Syllables
1.0–1.99	1	3
2.0–2.99	2	10
3.0–3.99	4	9
4.0–4.99	27	22
5.0–5.99	43	51
6.0–6.99	37	23
7.0–7.99	6	2

that their standard deviations given in Tables IV and V necessarily measure relative or ordinal variability, and are not influenced by the difference in the relative difficulty of the two types of material.

TABLE VII

THE COEFFICIENT OF VARIABILITY FOR EACH ITEM OF EACH LIST

Number of the Item	Words Lists I	III	V	VII	IX
1	69.22	38.74	76.84	31.81	54.55
2	68.15	54.70	100.46	50.20	47.76
3	51.35	52.22	60.38	53.95	41.59
4	53.05	49.40	46.22	50.78	43.10
5	50.96	60.71	42.71	48.22	51.71
6	50.33	41.99	43.45	43.68	49.91
7	40.79	45.82	52.61	50.12	37.01
8	51.30	37.19	27.89	47.46	71.06
9	50.00	49.24	42.34	37.81	36.99
10	50.68	50.71	40.16	44.41	50.78
11	21.19	36.04	54.07	42.49	52.91
12	16.49	49.34	48.45	50.21	53.93
13	47.12	51.38	46.17	31.73	41.50
14	47.75	35.95	33.97	37.52	40.55
15	61.23	47.54	39.87	38.55	47.75
16	63.48	47.68	27.33	43.35	38.54
17	41.96	46.42	37.24	54.02	34.51
18	52.31	44.21	36.18	42.50	44.38
19	49.01	33.74	38.67	39.42	36.96
20	44.31	44.98	45.03	35.67	38.25
21	46.47	71.89	57.99	38.94	26.67
22	45.24	49.19	58.47	50.85	50.04
23	32.62	42.23	39.22	45.64	49.74
24	40.61	40.19	46.59	35.58	49.57
Total	1135.62	1121.50	1142.31	1044.91	1089.76

TABLE VIII

THE COEFFICIENT OF VARIABILITY FOR EACH ITEM OF EACH LIST

Number of the Item	Syllables Lists II	IV	VI	VIII	X
1	65.87	57.40	18.25	85.75	66.01
2	65.78	72.90	17.25	74.62	62.56
3	59.54	70.00	21.33	64.67	66.91
4	48.24	56.12	58.20	51.78	67.34
5	65.80	51.23	49.16	52.28	22.26
6	40.68	54.27	38.44	56.75	43.17
7	40.90	33.53	36.41	76.00	34.63
8	59.15	30.91	27.54	32.05	43.35
9	43.35	28.58	42.44	41.90	17.97
10	36.35	28.28	42.95	40.90	29.88
11	40.80	42.84	49.53	38.08	38.68
12	39.98	30.24	38.24	37.13	22.83
13	39.37	36.43	41.46	125.18	30.74
14	38.14	48.14	28.44	45.18	51.25
15	43.09	25.22	49.07	39.39	50.45
16	36.59	35.75	51.73	38.46	47.22
17	39.93	29.32	50.29	51.29	28.52
18	31.00	24.57	56.02	20.11	42.63
19	21.33	21.25	45.41	31.02	49.81
20	36.39	44.57	38.18	34.70	41.46
21	49.54	50.10	26.45	49.59	47.50
22	38.09	65.60	20.22	54.39	54.02
23	25.90	31.14	46.99	39.18	45.16
24	41.87	52.53	43.88	54.69	66.00
Total	1042.68	1021.02	937.86	1235.13	1060.35

However, we also computed the coefficient of variability for each item. These are listed in Tables VII and VIII and the frequency distributions for the two materials are given in Table IX. Again, the syllables have the larger number of

TABLE IX

FREQUENCY DISTRIBUTION FOR THE COEFFICIENT OF VARIABILITY

Coefficient of Variability	Words	Syllables
15.0– 19.99	1	3
20.0– 24.99	1	8
25.0– 29.99	3	11
30.0– 34.99	6	10
35.0– 39.99	21	19
40.0– 44.99	23	17
45.0– 49.99	26	15
50.0– 54.99	28	14
55.0– 59.99	2	8
60.0– 64.99	3	2
65.0– 69.99	2	7
70.0– 74.99	2	3
75.0– 79.99	1	1
80.0– 84.99	0	0
85.0– 89.99	0	1
90.0– 94.99	0	0
95.0– 99.99	0	0
100.0–104.99	1	0
105.0–109.99	0	0
110.0–114.99	0	0
115.0–119.99	0	0
120.0–124.99	0	0
125.0–129.99	0	1

small values, the smaller number of intermediate values, and somewhat the greater number of larger values. Again, the data indicates the lesser variability of the syllables but the difference is slight and its reliability may be open to question.

There is no evidence that the amount of practice involved influenced in any decisive manner either the standard deviations or the coefficients of variability for either type of material. This conclusion is based upon the sums of the values for the various items in each list recorded at the bottom of each column (Tables IV, V, VII, VIII).

(3) ORDER OF LEARNING

Our records furnish some data upon a subsidiary topic which has often been investigated, namely, the temporal order in which the items of a list are memorized.

The mean values for each item of the five lists of words and the five lists of syllables are given in the last columns of Tables II and III respectively, and these values are represented by the unbroken lines in Figures 1 and 2 respectively.

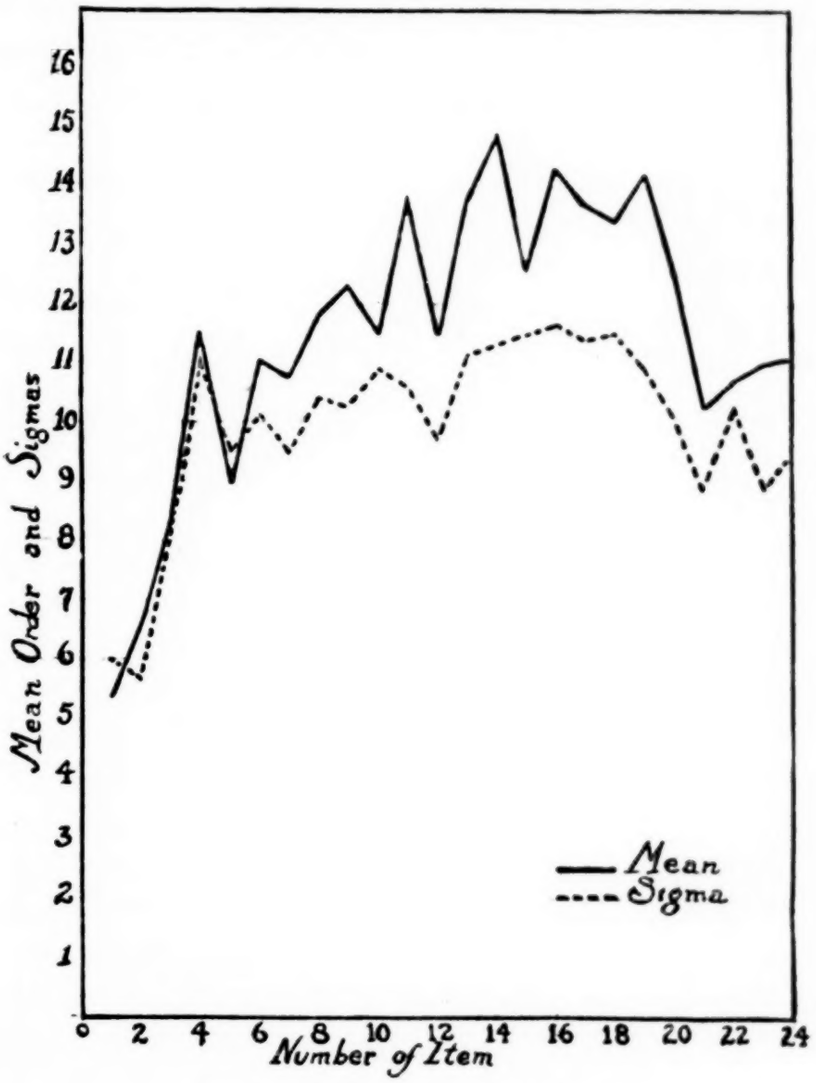

FIG. 1. Order of learning of words.

The order in which the items are learned is the same for both materials. The initial syllables are learned first, the final syllables next, while the last syllables to be learned are those coming just beyond the middle of the list. These results conform to the usual findings of previous experimenters. However, our data are based upon the rank order and not upon the number of trials as is usually the case, and

hence, the slope of our curves is not comparable with the slope of curves based upon the mean number of trials required for learning. It may also be noted that the syllables exhibit much the wider range of mean scores. The highest score for the nonsense syllables is slightly larger than the highest score of the words, while the scores for the first and last

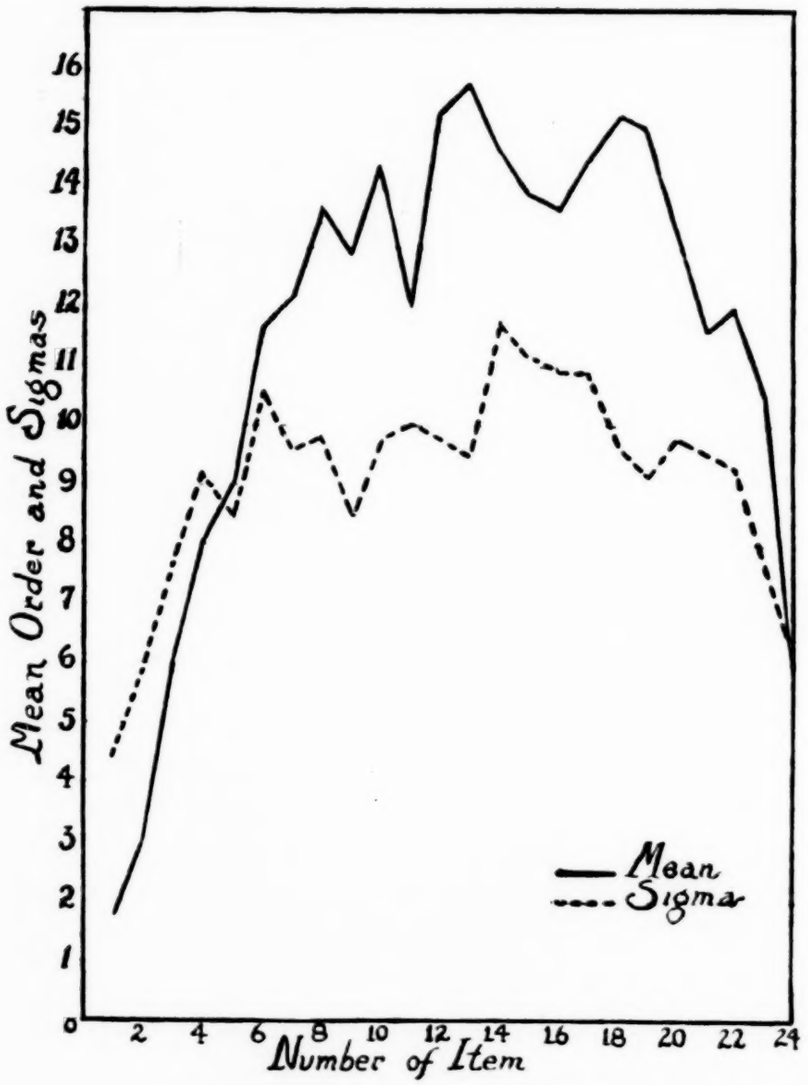

FIG. 2. Order of learning of syllables.

items of the lists of syllables are much lower than in the case of words. This fact may be due to the greater variability of the words, for the greater variability of the first item to be learned would obviously raise the mean score, while the greater variability of the last item to be learned would tend

to lower its mean score. On the basis of the above assumption, we would need to assume that the difference in the variability of the two materials is greatest for the items first to be learned and as a matter of fact, the nonsense syllables do exhibit the lesser variability for these items. We may also call attention to the fact that the graph for the nonsense syllables represents more accurately the influence of the positional factor in so far as the order in which these items are learned is less subject to individual differences.

The averages of the five standard deviations for each item of the two materials are given in the last columns of Tables IV and V. These data are graphically represented by the broken lines of Figures 1 and 2. It may be noted that these curves representing the average of the standard deviations of the scores for each item conform in shape to the graphs representing the corresponding means. The mean scores and their standard deviations are thus positively correlated. That is, the easier items, or the items first to be learned exhibit the least variability of score, while the hardest items, or those last to be learned, exhibit the greater range of individual variability. This relation is not a spurious correlation for these standard deviations measure relative or ordinal variability, and are not influenced by differences in the size of the mean score.

In conclusion we find that in the case of the individual variability of lists, the words are more variable than syllables. These results are in harmony with the Ebbinghaus doctrine. In the case of the individual variability of items, the same results are found, but the reliability of the difference may be open to question. The question can be raised as to why the variability is so much greater for words from list to list, than from item to item. The writer does not believe there is any inconsistency in the results, for in the one case, the list was taken as a whole and the number of trials was taken into consideration. In the other case, we dealt with the ordinal position inside the list. Concerning the order of learning of both types of materials, the results are the same as have usually been found by previous experimenters;

namely, the initial syllables are learned first, final syllables next, while the last syllables to be learned are those coming just beyond the middle of the list. The variability of the individual scores varies directly with the order of learning.

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A FURTHER STUDY OF THE EFFECTS OF LOSS OF SLEEP

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It is comparatively easy, as Morgan [1] points out, to measure muscular fatigue. It is a relatively simple matter to isolate a muscle or group of muscles and observe any effect upon them of long continued and steady exercise. The measurement of mental fatigue is not so simple, if, after all, any measurement is possible. The question, what is fatigue, is one which has not yet been satisfactorily answered, and the popular use of the term detracts from rather than adds to its scientific worth. We are rather inclined to the opinion of Shelley & English [2] who hold that fatigue is a condition rather than a tangible factor, much the same to psychology as "fever" is to medicine. Of much the same mind is Myers [3] in regarding fatigue as the breaking down of inhibitions, so that any inefficiency observed during a state of tiredness is due not to fatigue as a quality, but to the inability to adjust to conditions readily; holding that in muscular work it is the "process of adaptation which finally tires. In mental work it is the inability to maintain the right attitude which finally tires." Being engaged in mental work, all conflicting nervous impulses must be inhibited. Such inhibition involves effort, and fatigue, it seems, lessens the ability to preserve the proper attitude toward work.

This study was carried on much along the lines of a previous study,[4] though on a larger scale, and has for its purpose the measurement in as objective manner as possible the effects of fatigue induced through the loss of sleep on

[1] J. J. B. Morgan, Effect of Fatigue on Attention, this JOURNAL, 1920, 3, 319–333.

[2] M. Shelley, and H. B. English, The Effects of Fatigue on Learning, *Amer. J. of Psychol.*, 1926, 37, 429–430.

[3] C. S. Myers, The Study of Fatigue, *J. of Per. Res.*, 1925, 3, 321–339.

[4] T. F. Weiskotten, On the Effects of Loss of Sleep, this JOURNAL, 1925, 8, 363–380.

the learning process. It aims, (1) to determine the immediate effects of loss of sleep, and (2) to determine the remote or after effects upon the mental processes involved in different kinds of learning. Although the methods of scoring are objective, it is necessary at times to make use of the subjects' introspections in the interprepation of the results; and while it is realized that this study does not demonstrate what fatigue is, nor measure the neurological changes which probably take place, it does aim to demonstrate what fatigue does.

The field of experimentation is replete with work done in this connection, but as yet, not a great deal has been done in the way of inducing fatigue through loss of sleep. The work of Miss Arai [5] is by far the best and most complete of its kind, but her methods of inducing fatigue did not include that of insomnia. Industrial and business journals are full of investigations of this sort, among which the findings of Martin,[6] Mayo,[7] Myers,[8] are but few. Painter,[9] Shelley & English,[10] and Whiting & English [11] make their approach from the educational point of view, but their method is also that of long continued activity. Patrick & Gilbert,[12] Smith [13] and Robinson & Herrmann,[14] to whose works reference has been made in the previous study already noted, used the loss of sleep as the essential element for producing fatigue and point strongly to a loss of efficiency during periods of insomnia. Their findings stand in marked contrast with the opinion of

[5] T. Arai, Mental Fatigue, *T. C. Cont. to Educ.*, Columbia Univ., 1912, 54.

[6] E. G. Martin, Fatigue and Rest, *Indust. Psychol.*, 1926, 1, 345–353.

[7] E. Mayo, Reverie and Industrial Fatigue, *J. of Per. Res.*, 1924, 3, 273–281.

[8] C. S. Myers, *op. cit.*

[9] W. S. Painter, Efficiency in Mental Multiplication with Extreme Fatigue, *J. of Educ. Psychol.*, 1916, 7, 25–31.

[10] *Op. cit.*

[11] H. F. Whiting, & H. B. English, Fatigue Tests and Incentives, this JOURNAL, 1925, 8, 33–49.

[12] G. T. W. Patrick, & J. A. Gilbert, On the Effects of Loss of Sleep, *Psychol. Rev.*, 1896, 3, 469–483.

[13] M. Smith, A Contribution to the Study of Fatigue, *Brit. J. of Psychol.*, 1916, 8, 327–350.

[14] E. S. Robinson, & S. O. Herrmann, Effects of the Loss of Sleep, this JOURNAL, 1922, 5, 93–100.

Laird [15 and 16] and with his statement, "When a battery of short mental tests is given to subjects who have been without sleep for one, two and in some cases three nights, no appreciable and consistent decrease in performance is reported." Nor is his statement consistent with his findings. Using two groups, a control and an experimental group, he administered problems in mental multiplication for thirty minute periods until the practice factor had been eliminated. The experimental group then went for one night without sleep. In every case more errors were made by the experimental group on the day following the lack of sleep. While the inefficiency was not great, it is at least significant, especially considering the brevity of the sleepless period. Furthermore, the introspections of his subjects agree very closely with those of others, viz., loss of concentration, lack of attention, difficulty in visualization, wandering of attention, nervousness, accentuation of noises, lack of inhibition of muscular activity, giddiness and dizziness.

In his second investigation with Wheeler assisting, he varied his method and his measurements in an effort to determine the amount of nervous energy consumed. Using three subjects, he allowed them several weeks' practice in mental multiplication, and, after the effects of practice had worn off, the tests were given for one week during which the usual amount of sleep was taken (the control phase). Following this was a week when the amount of sleep was curtailed two hours per day by retiring that much later (the experimental phase). During this week, immediately upon awaking, a five minute sample of each subject's expired air was taken. This was followed by ten minutes of mental multiplication before arising, after which another five minute sample of expired air was obtained. The analysis of the expired air served to measure the nervous energy consumed which was found to be greater in every case after a loss of sleep.

[15] D. A. Laird, Effects of Loss of Sleep on Mental Work, *Indus. Psychol.*, 1926, 1, 427–428.

[16] D. A. Laird, & W. Wheeler, What It Costs to Lose Sleep, *Indus. Psychol.* 1926, 1, 694–696.

Method Of Experimentation

Five subjects were used in this study, three forming an experimental group and two a control group. The subjects are designated by letter: Subject A, female, undergraduate student; subject B, female, undergraduate student; and subject C, male, graduate student, formed the experimental group. Subject D, female, graduate, and subject E, male, graduate, formed the control group. All subjects were acquainted with the experimental method.

Three primary tests were employed, designated as tests No. 1, No. 2, and No. 3. The first was a test of purely motor skill, ball tossing. Subjects were provided with hard rubber balls, one inch in diameter; with baskets measuring one foot in height and six inches in diameter. From a distance of twelve feet these balls were tossed underhand toward the basket situated against a smooth wall. Only clean hits were allowed, bank shots from the wall being discredited. The score was the number of balls tossed fairly into the basket in fifty trials.

Test No. 2 was one of sensori-motor learning. Pencil, paper, a copy of the Morse Telegraphic Code and a copy of "The Story of Philosophy" by Will Durant were provided each subject. At the signal, "Ready—Begin," the subjects transposed for a five-minute period from the Introduction of Durant's book the context of the page into the telegraphic code. Instructions were to work as rapidly as possible but to be sure that no mistakes were made. The score was the number of letters transposed in the five-minute period, time being recorded by means of a stop watch. The subjects began at the same point in the context on each successive trial. The reason for the selection of this material was the legibility of the print, the length of the paragraphs making for less mental confusion, and the uniformity of the material throughout, minimizing any difference in difficulty.

Test No. 3 was one of abstract learning in which one two place number was multiplied by another two place number mentally. Ten such problems constituted one trial, the score being the number of seconds required to mentally multiply

one trial of ten such problems each. A record was also kept of the percent of accuracy of each trial and the number of errors made. Record was also kept of the pulse rate, body weight and body temperature of the experimental group throughout the experiment.

The experiment was divided into three phases, the pre-insomnia phase of 18 days duration, the insomnia phase of approximately 3 days duration, and the post-insomnia phase of 6 days duration. The tests were given to the experimental group once each day during the pre-insomnia and post-insomnia phases. On the nights of the 18th, 19th and part of the 20th day no sleep was taken by the experimental group and during this time tests were administered every six hours beginning at 10 A.M. on the 18th day. The control group took the tests in the same manner except that during the insomnia phase they were given but one test per day at 10 A.M. The period of insomnia began at 7 A.M. on the 18th day and ended at 1 A.M. on the 21st day, a period of 66 hours.

Throughout the entire experiment, introspections were made at any time need was felt for recording desirable notes, regarding the mental processes, mental attitudes, physical conditions, feelings, general impressions, etc. Although no rules were enforced and no regulations imposed except those involved in the administration of the tests, regularity in living was encouraged. All subjects were in good physical condition at the beginning of and throughout the experiment. During the period of insomnia, subjects A and B remained awake together. Subject C was alone.

Results

The results of the tests from the beginning to the end of the testing periods are given in Tables I, II, and III. Table IV gives the records for temperature, pulse rate and body weight for the experimental group, the two subjects in the control group making no record of physical condition.

A study of the scores of test No. 1 given in Table I indicates considerable individual differences, which are the more

noticeable in the curves of Graph I. The rise in all curves is gradual and in no case is the difference between the initial and end test great. Often the lowest scores were made late in the experiment after considerable practice had been gained. Introspective reports of all subjects agree that the learning

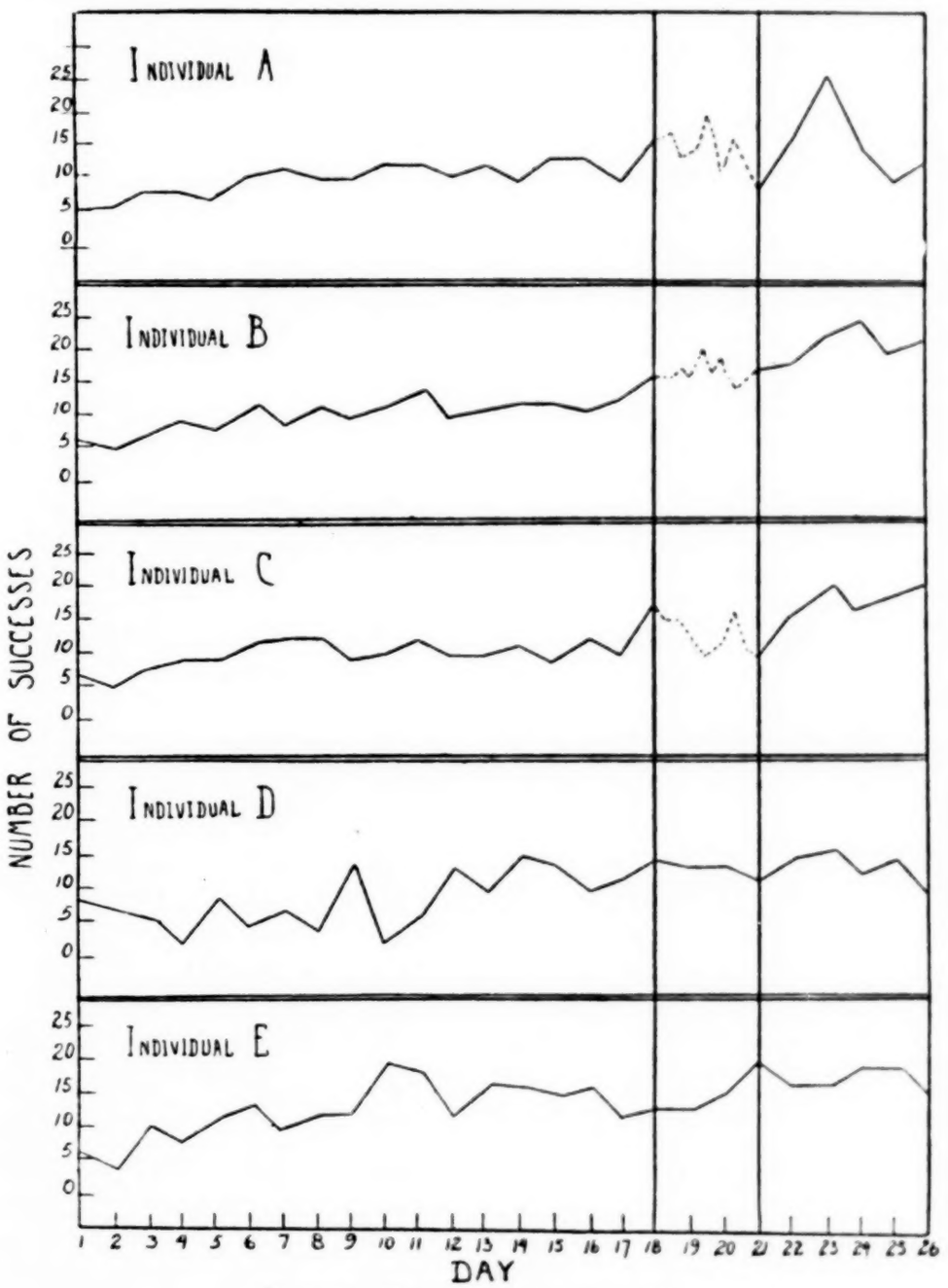
TABLE I

RESULTS FROM MOTOR LEARNING TESTS—BALL TOSSING

Scores in Number of Successes in Fifty Throws, Day by Day for Pre-Insomnia, Insomnia, and Post-Insomnia Periods

	Test No.	Day and Time of Test	Individuals A	B	C	D	E
Pre-Insomnia Period	1	1st da.	5	6	7	8	6
	2	2d da.	6	5	5	7	4
	3	3d da.	8	7	7	6	10
	4	4th da.	8	9	9	2	8
	5	5th da.	7	8	9	8	11
	6	6th da.	10	11	11	5	13
	7	7th da.	11	9	12	7	10
	8	8th da.	10	11	12	4	12
	9	9th da.	10	10	9	13	12
	10	10th da.	12	11	10	3	19
	11	11th da.	12	13	12	6	18
	12	12th da.	11	10	10	13	12
	13	13th da.	12	11	10	10	16
	14	14th da.	10	12	11	15	16
	15	15th da.	13	12	9	14	15
	16	16th da.	13	11	12	10	16
	17	17th da.	10	13	10	12	12
	18	18th da.	15	16	17	15	13
		Mean score	10.2	10.3	10.1		
Insomnia Period	19	18 da. 10 P.M.	17	16	15		
	20	19 da. 4 A.M.	13	17	15		
	21	19 da. 10 A.M.	14	16	13	14	13
	22	19 da. 4 P.M.	15	18	12		
	23	19 da. 10 P.M.	19	20	10		
	24	20 da. 4 A.M.	16	17	11		
	25	20 da. 10 A.M.	11	19	12	14	15
	26	20 da. 4 P.M.	16	15	16		
	27	20 da. 10 P.M.	14	14	13		
		Mean score	15.0	16.9	13.0		
Post-Insomnia Period	28	21st da.	11	17	10	12	20
	29	22d da.	15	18	16	15	17
	30	23d da.	25	22	19	16	17
	31	24th da.	17	24	17	13	19
	32	25th da.	10	20	18	15	19
	33	26th da.	13	21	19	10	16
		Mean score	15.2	20.3	16.7		
	Mean for the series		12.4	13.9	11.2	10.4	13.8

was of a purely mechanical nature with little or none of the ideational element. Feeling and attitude entered in strongly and mental set played an important part. The kinesthetic element also was predominant. Loss of sleep seems in no way to have affected the performance of any individual. While, during the sleepless period, fluctuations in scores were great, they are not much more marked than at any other



GRAPH I. Curves for motor learning.

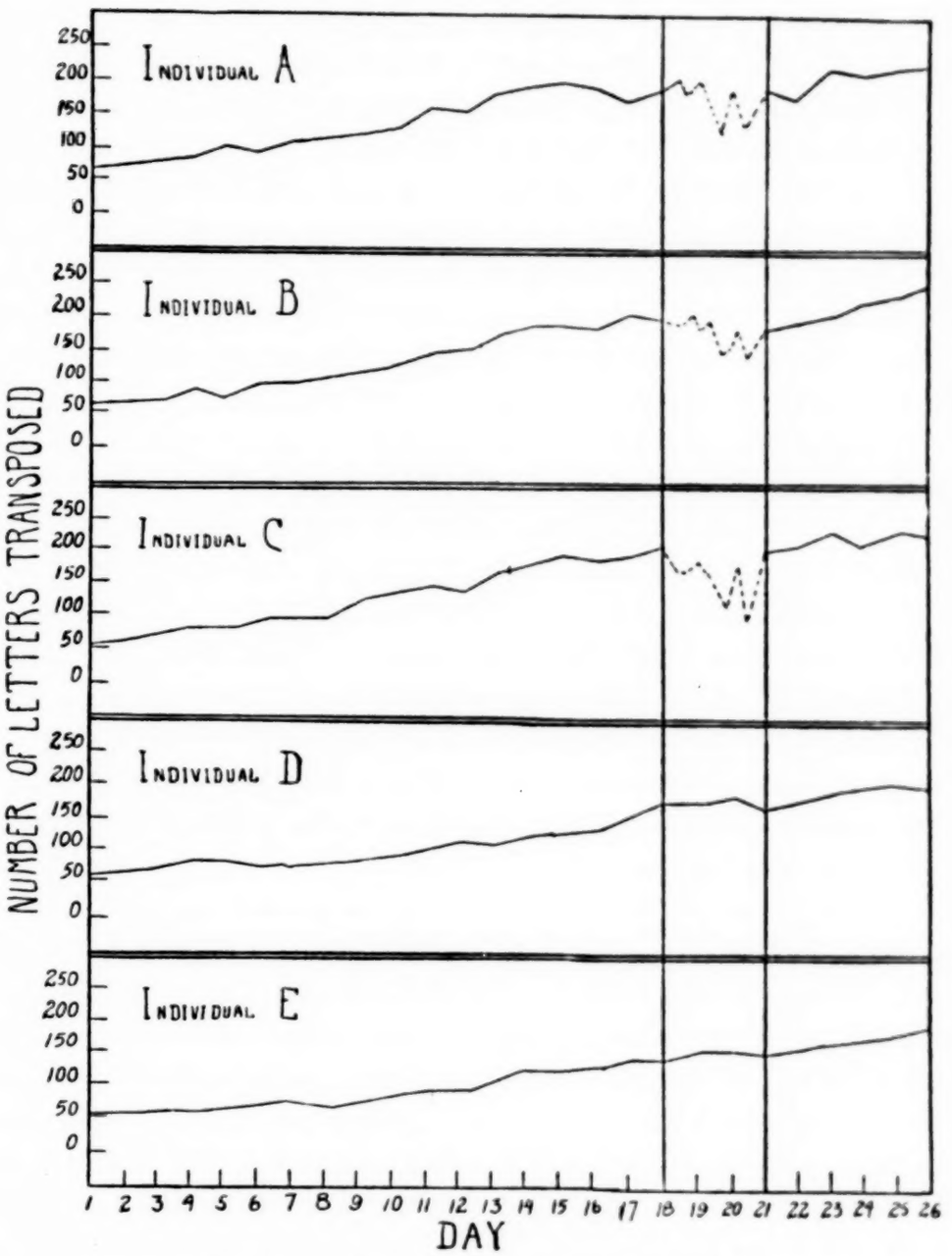
time, nor were any exceptionally low scores made during that time. This may be due to the nature of the test, since the real effects of insomnia appeared to be mental rather than physical. This explanation seems to be all the more plausible when the kinesthetic habits formed are considered.

TABLE II

RESULTS FROM SENSORI-MOTOR LEARNING TESTS—SUBSTITUTION

Scores in Number of Letters Transposed in Five Minutes, Day by Day for Pre-Insomnia, Insomnia and Post-Insomnia Periods

	Test No.	Day and Time of Test	Individuals A	B	C	D	E
Pre-Insomnia Period	1	1st da.	68	61	54	62	54
	2	2d da.	72	68	63	66	58
	3	3d da.	79	68	72	74	63
	4	4th da.	88	87	86	85	61
	5	5th da.	97	77	84	85	69
	6	6th da.	93	96	99	76	73
	7	7th da.	107	98	93	80	76
	8	8th da.	112	105	98	83	70
	9	9th da.	120	111	120	85	79
	10	10th da.	133	129	139	92	87
	11	11th da.	153	140	149	102	95
	12	12th da.	153	145	138	117	93
	13	13th da.	183	171	163	111	106
	14	14th da.	195	187	179	122	126
	15	15th da.	200	189	192	129	124
	16	16th da.	192	185	189	136	130
	17	17th da.	175	203	193	151	143
	18	18th da.	192	196	201	175	143
		Mean score	134.0	127.8	128.4		
Insomnia Period	19	18 da. 10 P.M.	207	191	175		
	20	19 da. 4 A.M.	182	200	175		
	21	19 da. 10 A.M.	201	187	184	177	152
	22	19 da. 4 P.M.	196	191	176		
	23	19 da. 10 P.M.	184	181	160		
	24	20 da. 4 A.M.	137	150	121		
	25	20 da. 10 A.M.	187	159	143	188	156
	26	20 da. 4 P.M.	177	174	180		
	27	20 da. 10 P.M.	138	147	101		
		Mean score	178.8	175.6	156.7		
Post-Insomnia Period	28	21 da.	198	182	202	174	153
	29	22 da.	184	195	213	182	161
	30	23 da.	228	206	231	198	168
	31	24 da.	218	223	217	205	175
	32	25 da.	221	239	232	207	183
	33	26 da.	231	242	228	201	196
		Mean score	213.3	214.5	220.5		
Mean for the series			160.6	157.0	152.9	129.3	114.8



GRAPH II. Curves for sensori-motor learning.

The tossing of the balls became toward the end of the experiment to be done wholly in accord with the "feel" of the muscles involved, and subject C found that he was able to score approximately the same number of successes with his eyes closed as he was able to do with them open—this at the end of the experiment.

Table II and Graph II giving the scores from test No. 2

tell a different story. Individual differences are again noted, especially with reference to individual speeds of transposition, although the curves for the five subjects, the sleepless period excepted, are approximately the same. We would account for the consistently lower scores of subjects D and E by the fact that lack of space forced them to assume a somewhat awkward position in taking these tests, writing with the material upon their laps instead of upon a desk as was the case with the experimental group. However, a steady rise is noticeable in all curves indicating a gradual learning of the telegraphic equivalent for the English letters. There appears to be no sudden solution of the problem. The range in increase of letters transposed per five minutes is from approximately 55 at the initial test to approximately 220 at the end test. Just before the beginning of the period of insomnia, all subjects felt that they had nearly reached their physiological limit. Every one knew the Morse Code so that there was no longer waste time in looking for telegraphic symbols. Fatigue was felt which rendered further speed difficult, although the scores of the post-insomnia period do show a slight increase in scores.

During the period in which no sleep was taken, a comparison of the scores and curves of the experimental group with the control group is of interest. Those of the experimental group show much fluctuation, gains and losses, rises and falls which appear to be significant, particularly the two low scores which in every case appear at the testing hours of 4:00 A.M. and 10:00 P.M. of the 20th day. It was at these two times during that period when the influences of insomnia were most keenly felt, all of which agrees with the report of Patrick & Gilbert [17] when they state that it was comparatively easy to remain awake during the day, but that during the night a well-nigh irresistible impulse to sleep came on. This is the common report of all subjects in this experiment also. Concentration was at a minimum. Much waste movement, loss of memory for some of the most-used letters and inability to attain speed are reported. The

[17] G. T. W. Patrick, & J. A. Gilbert, *op. cit.*

greatest loss of efficiency is noticeable in the scores of Individual C, which may be accounted for by the absence of the stimulating effect of companionship present in the cases of Individuals A and B. The immediate return to normal reactions after sleep had been taken is significant and will be noted later.

Much the same situation is to be seen in a study of Table III and Graph III showing the results for the tests in abstract learning in mental multiplication. Here in general the curves show a rather rapid fall from the initial period until the seventh or eighth day. Mental multiplication presented to every subject involved a newer variety of aspects than either of the other tests, and the scores and introspections indicate a process of learning and adjustment with a touch of the "problem-solving" type of procedure. Between the fifth and the tenth days of the experiment, rapid falls are to be noted in the curves of all individuals and the introspective notes point toward the influence of the discovery of methods of solution which were used for the remainder of the testing period. Again, a comparison of the curves of the experimental group with those of the control group during the period of insomnia indicates the same as in Table II, a rapid loss of efficiency toward the end of the sleepless period with the greatest loss at the hours of 4:00 A.M. and 10:00 P.M. on the 20th day. Again this loss is more marked in the case of Individual C than in the cases of A and B who shared each other's company.

Although the method employed by Painter,[18] who mentally multiplied one four place number by another four place number for more than three and a half hours, differs from this procedure, we found nothing which would suggest his statement that "there exists a definite and relatively abrupt appearing point beyond which mental work becomes impossible." Rather do the findings agree with those of Arai,[19] whose subjects mentally multiplied on four consecutive days for twelve hour periods, showing a gradual decrease in speed

[18] W. S. Painter, *op. cit.*, p. 31.

[19] T. Arai, *op. cit.*

TABLE III

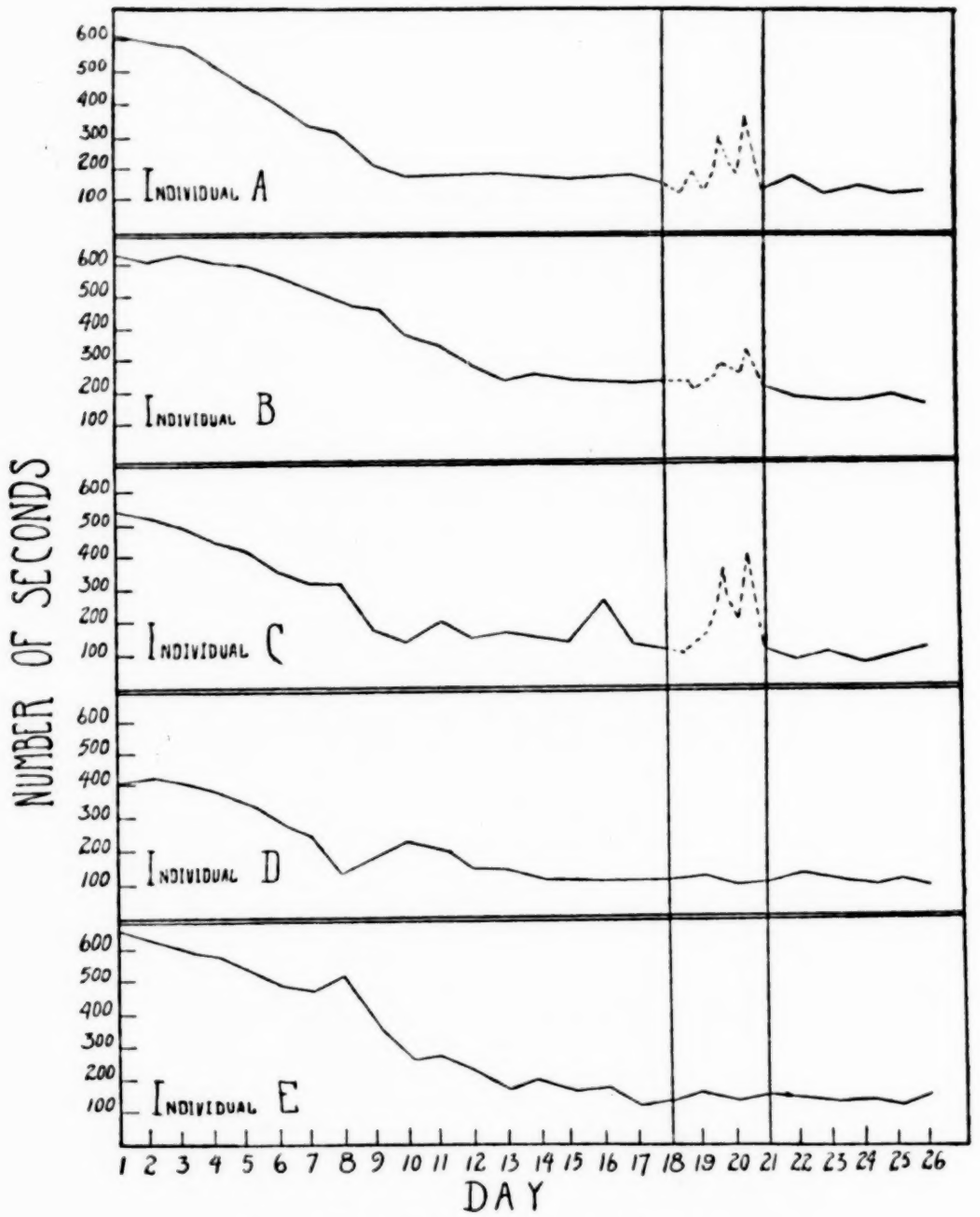
Results from Abstract Learning—Mental Multiplication

Scores in Seconds, Percent of Accuracy, Number of Errors, Day by Day for Pre-Insomnia, Insomnia, and Post-Insomnia Periods

			Individual A			Individual B			Individual C			Individual D			Individual E		
	Test No.	Day and Time of Test	No. Sec.	No. Errors	Acc'y %	No. Sec.	No. Errors	Acc'y %	No. Sec.	No. Errors	Acc'y %	No. Sec.	No. Errors	Acc'y %	No. Sec.	No. Errors	Acc'y %
Pre-Insomnia Period	1	1st da.	611	9	30	623	5	50	540	4	70	401	7	30	650	3	70
	2	2d da.	598	3	70	617	4	70	526	1	90	428	3	70	627	4	80
	3	3d da.	581	4	60	632	1	90	493	0	100	418	4	60	601	2	80
	4	4th da.	520	3	80	611	3	70	458	4	60	387	1	90	582	1	90
	5	5th da.	461	0	100	598	2	80	428	6	50	358	3	80	539	3	80
	6	6th da.	402	2	80	569	4	70	360	8	30	294	5	70	497	5	70
	7	7th da.	340	1	90	532	6	40	320	3	70	254	6	60	475	6	60
	8	8th da.	310	9	30	497	3	80	285	4	70	133	10	30	520	1	90
	9	9th da.	215	2	80	465	0	100	185	0	100	183	4	70	382	1	90
	10	10th da.	182	1	90	392	2	80	150	6	60	239	5	70	270	4	70
	11	11th da.	180	1	90	349	4	60	200	2	80	210	3	70	277	1	90
	12	12th da.	185	1	90	297	0	100	163	6	50	157	2	90	238	7	50
	13	13th da.	185	2	80	251	1	90	175	8	30	148	4	80	178	2	90
	14	14th da.	180	1	90	278	1	90	160	3	70	121	2	80	200	2	80
	15	15th da.	175	0	100	267	0	100	146	4	60	115	0	100	176	0	100
	16	16th da.	180	3	80	251	2	80	255	6	60	108	3	90	177	0	100
	17	17th da.	182	2	80	249	1	90	143	1	90	109	3	90	125	0	100
	18	18th da.	162	4	60	257	0	100	126	1	90	113	1	90	131	3	80
		Mean score	313.8	2.7	77	429.1	2.2	81	298.6	3.7	68						

TABLE III—(*Continued*)

	Test No.	Day and Time of Test	Individual A			Individual B			Individual C			Individual D			Individual E		
			No. Sec.	No. Errors	Acc'y %	No. Sec.	No. Errors	Acc'y %	No. Sec.	No. Errors	Acc'y %	No. Sec.	No. Errors	Acc'y %	No. Sec.	No. Errors	Acc'y %
Insomnia Period	19	18 da. 10 P.M.	137	1	90	250	2	80	119	0	100						
	20	19 da. 4 A.M.	194	0	100	241	0	100	138	2	80						
	21	19 da. 10 A.M.	165	0	100	247	3	70	145	0	100	134	1	90	160	0	100
	22	19 da. 4 P.M.	155	0	100	251	2	90	184	2	80						
	23	19 da. 10 P.M.	206	3	70	260	0	100	229	1	90						
	24	20 da. 4 A.M.	315	1	90	298	4	70	365	2	80						
	25	20 da. 10 A.M.	231	4	60	293	0	100	276	5	70	107	8	60	141	8	40
	26	20 da. 4 P.M.	199	1	90	276	2	90	222	1	90						
	27	20 da. 10 P.M.	360	6	70	322	3	80	404	4	80						
		Mean score	218	1.8	86	270.9	1.8	87	231.4	1.9	86						
Post-Insomnia Period	28	21 da.	149	7	60	228	3	80	124	2	80	109	0	100	151	3	70
	29	22 da.	183	0	100	202	0	100	101	1	90	127	0	100	142	2	80
	30	23 da.	121	2	90	190	1	90	118	0	100	111	1	90	137	0	100
	31	24 da.	151	1	90	189	0	100	90	1	90	104	3	70	140	0	100
	32	25 da.	130	1	90	209	5	70	106	0	100	108	2	90	135	1	90
	33	26 da.	140	2	80	191	4	80	129	4	70	101	3	80	142	0	100
		Mean score	145.5	2.1	85	201.5	2.1	87	111.3	1.3	88						
	Mean for the series		255.6	2.3	81	344.5	2.1	83	240.2	2.8	77	195.3	3.2	77	299.7	2.3	83



GRAPH III. Curves for abstract learning.

due to poor association, lack of concentration and wandering of the mind. Poor concentration was felt by all in this case to be the factor which predominated, inability to fix the attention long enough to complete one unit of work. In this we agree with Whiting and English that "fatigue is a conscious (if negative) motive to action. Fatigue does not directly cause work decrement but raises the threshold at which work motives are effective." [20]

[20] *Op. cit.*, p. 49.

The Graph gives no indication of the amount of accuracy or the number of errors which Table III records. The chief concern was with the time taken by each individual to be reasonably sure that he had arrived at the correct solution of each problem; but it is to be noticed from Table III as of significance that accuracy is not less but greater in most cases during the period of insomnia, a condition which appeared to hold in the previous report.[21] It would seem that the loss of sleep does not affect in any serious way the accuracy of an individual during the brief and sporadic intervals during which work motives are operative.

Table IV, giving the readings for body temperature, body-weight and pulse rate reveals nothing from which any conclusions might be drawn. No undue fluctuations in any of the records are to be found. Temperatures of all subjects maintain themselves well within the normal limits, and although in the case of Individual C, on four successive tests during the period of insomnia, and once in the case of Individual A, temperatures did drop below the 98 degree mark, the variations are so slight and the fact that at the end of the sleepless period there was a return to normal, there is no real ground for attributing this fluctuation to the loss of sleep. In the previous experiment of the writer no records were made of body temperature, but a feeling of bodily coldness is reported together with an extreme lowering of the pulse rate[22] indicating that the bodily temperature might have been affected. This previous experiment, it should be borne in mind was carried on during the winter months and this present investigation during the early summer allowing for the possibility that the season of the year may be a variant for which reckoning must be made.

With respect to the pulse rates of the individuals, in spite of the fact that low rates are recorded in every case during the sleepless period, we do not feel that it can be said with any degree of assurance that such lowerings of the pulse-rate are directly attributable to the loss of sleep although

[21] T. F. Weiskotten, *op. cit.*

[22] P. 369.

TABLE IV

Physical Data

Readings for Temperature, Pulse-rate and Body Weight, Day by Day for Pre-Insomnia, Insomnia, and Post-Insomnia Periods

	Test No.	Day and Time of Test	Temperature Ind. A	Temperature Ind. B	Temperature Ind. C	Pulse-rate Ind. A	Pulse-rate Ind. B	Pulse-rate Ind. C	Body Weight Ind. A	Body Weight Ind. B	Body Weight Ind. C
Pre-Insomnia Period	1	1st da.	98.2	98.6	98.6	76	74	76	134	128	131¼
	2	2d da.	98.6	98.6	98.4	72	74	78	133¾	127½	131½
	3	3d da.	98.6	98.2	98.6	68	78	82	132½	127¾	131¼
	4	4th da.	98.0	98.0	98.0	72	80	82	133¾	127	132
	5	5th da.	98.4	98.2	98.4	80	76	78	133½	127½	131½
	6	6th da.	98.6	98.4	98.0	82	72	84	132	128	131¾
	7	7th da.	98.2	98.6	98.6	72	74	82	133	128¼	131½
	8	8th da.	98.2	98.4	98.0	66	88	82	133¾	128	131¼
	9	9th da.	98.2	98.0	98.0	70	76	82	133¼	129	131¼
	10	10th da.	98.6	98.6	98.0	72	80	82	133½	128¾	132¼
	11	11th da.	98.0	98.4	98.1	66	82	82	134	128¼	132
	12	12th da.	99.4	98.6	98.0	72	78	82	132½	128	132¼
	13	13th da.	99.2	98.2	98.0	68	78	84	133½	127½	130¼
	14	14th da.	98.4	98.0	98.1	60	72	96	132	128	131
	15	15th da.	98.2	98.6	98.2	76	74	82	133½	128¾	131¼
	16	16th da.	98.2	98.6	98.0	78	80	88	134	129	130
	17	17th da.	98.6	98.4	98.0	82	82	86	133½	128½	131
	18	18th da.	98.6	98.6	98.0	72	84	80	133	128	130
Insomnia Period	19	18 da. 10 P.M.	98.6	98.2	98.0	72	84	88			
	20	19 da. 4 A.M.	98.8	98.6	98.0	78	82	80			
	21	19 da. 10 A.M.	98.0	98.6	98.4	76	76	80	130½	127¾	128
	22	19 da. 4 P.M.	98.6	98.0	98.0	84	74	76			
	23	19 da. 10 P.M.	98.6	98.4	97.2	88	72	76			
	24	20 da. 4 A.M.	98.0	98.0	97.2	60	72	78			
	25	20 da. 10 A.M.	97.6	98.6	97.1	58	70	70	132	127¾	129
	26	20 da. 4 P.M.	98.6	98.0	97.8	60	66	62			
	27	20 da. 10 P.M.	98.6	98.6	98.6	60	70	60			
Post-Insomnia Period	28	21st da.	98.6	98.6	98.6	72	78	78	132	128	129
	29	22d da.	98.6	98.4	98.0	78	80	86	132½	127½	128½
	30	23d da.	98.6	98.0	98.0	72	82	82	132	127	129½
	31	24th da.	98.0	98.0	98.0	72	76	84	131	128	130¼
	32	25th da.	98.0	98.2	98.0	64	80	94	131¼	128½	130½
	33	26th da.	98.0	98.6	98.2	64	84	82	132¼	128	131

insomnia may doubtless be one of the factors. It is to be noted that throughout the entire readings the pulse-rates of the subjects show considerable fluctuation from day to day, a condition which, in the opinion of many medical authorities, is not uncommon, but which is due to the mental and physical attitudes at the time the pulse is read. We simply mention this lowered pulse-rate during the insomnia period without feeling that there is sufficient ground for any definite statement here although this is in agreement with the findings of Arai and the former investigation of the writer. Arai [23] notes, however, the difference of opinion on this point referring to the reports of Davey, Gley, Billings and Shepard as substantiating a higher run of pulse-rate as fatigue grew, Vaschide and des Bancels a decrease, and Benedict and Carpenter as finding no change at all. Nor can any conclusions be drawn from the readings of body weight, the relative constancy of weight in every case being noticeable with no more than the usual slight variation from day to day even during the period of insomnia.

The introspections of the subjects, while not being presented as of scientific worth, are interesting and were referred to for further bases in the interpretation of the results. In general it might be said that in no case were any extremely severe reactions of a physical nature noted or felt, but this does not imply that the reactions reported were not distinctly and sharply defined. It does mean that the effects of fatigue came about gradually and without any of the violent manifestations experienced by the writer in his previous investigation, or such as those reported by Patrick and Gilbert [24] or Smith [25] who describes certain feelings as "positive pain;" nor was any evidence given of any mental acceleration during the earlier stages of the insomnia period such as were previously reported and what Smith [25] speaks of as "emotional belief in the power to conquer all things." Certain reactions were demonstrated by all of the subjects of the experimental group chief among

[23] *Op. cit.*
[24] *Op. cit.*
[25] *Op. cit.*, p. 337.

which was the state of nervousness and irritability which tended to increase as the sleepless period continued. There was marked accentuation of noises especially during the night hours, the sort of noises ordinarily not noticed, motors in the street, rustling of the leaves, the whistle of a distant train, footsteps, the ticking of the clock. A sandy feeling in the eyes was a common report, but perhaps the most significant of all the feelings reported by the entire group was the lack of inhibition displaying itself in a variety of subtle forms. We have already, in the discussion of the results, spoken of this lack of inhibition with respect to mental functioning demonstrating itself in the inability to concentrate. Thought drifted from one thing to another with no associative sequence, the first stimulus to attract attention being the one to receive attention until some other stimulus supplanted it. The mind apparently took the path of least resistance with the result that a state of lethargy was experienced. Such conditions are strongly suggested by Laird [26] whose subjects reported, 'no attention,' 'difficulty to visualize,' 'wandering of attention,' 'creaking of the building bothered,' and by Painter, Patrick and Gilbert, and Robinson. On the physical side, this lack of inhibition was displayed by a great amount of waste movement, 'unsteadiness of gait,' 'aimless wandering about the room,' 'repetition of movements' especially in working the substitution tests. With Individual A this reaction took the form of incoherent talking especially during the latter half of the second night without sleep, a condition described as being 'very much like semi-consciousness in which I was aware of the surroundings, of conversation and interrogation, but was unable to make a coherent reply, words without any conscious association with anything.' There was no report of feelings of giddiness or mild intoxication such as were previously reported by the writer and by Laird, several of whose subjects felt 'almost tite.'

In addition to these reactions which were experienced by the entire group, there were others which were reported by

[26] *Op. cit.*, p. 427.

one or two of the three. Subjects B and C report a heaviness of body, especially of the limbs and arms, which tended to increase as the sleepless period was prolonged without respect to time of day. Great effort was needed to physically motivate oneself, illustrated by the tendency to 'shuffle' while walking. Subjects B and C also experienced headache. Subject C had illusions during the second night of the insomnia period, objects within range of vision appeared 'to shift momentarily with respect to their spacial relation, this shifting being accompanied by a blankness of the mind so that it seemed that the shifting was the result of this condition.' The illusion 'served to dispel this blank state of mind and then the shifting ceased.' Conditions similar to this are reported by Patrick and Gilbert, and by Painter who, during the course of his mental multiplication, says that he felt himself to be two persons, the one working and the other looking on. It is also in agreement with the previous report of the writer who experienced conditions very much like trances in which a blankness of the mind was the prime feature. As the sleepless period progressed, Subjects A and B complained of a feeling of coldness. The fact that A and B report a feeling of general tiredness for three days following the period of insomnia is interesting and perhaps significant. Although the test scores of these subjects give no indication of this tired feeling, the fact that the only other report similar to this is that of Miss Smith [27] who experienced a continuation of the effects of loss of sleep for sixteen days following the taking of sleep, might indicate that there are sex differences which need further demonstration. From another point of view, as has already been indicated, neither Subject A nor B reacted as strongly to the influence of fatigue during the actual period of insomnia as did Subject C. It may be that the influence of companionship serves to stave off the immediate effects of fatigue which then tend to assert themselves at a later time.

A recognition of the many weaknesses in the technique and procedure, the limited number of subjects used, the inability at complete control of all variables will not allow

[27] M. Smith, *op. cit.*

one to attach too much weight to the records. Without according, also, too much significance to the personal reports of the subjects, the many points of agreement with the findings of others who have made investigations in this field certainly add weight and substantiation of an accumulative character. It is difficult to attribute the fluctuations in the curves during that period of sleeplessness to any other factor but the loss of sleep, especially when a comparison is made with the curves of the control group. The summary, then, which follows, is not to be interpreted in any sense as being dogmatic, but simply as statements of hypotheses, indication of which is derived from this investigation and the comparison of reports of others.

Summary

1. With respect to physiological effects of fatigue no marked indications were given.

2. The mental functions seem to be rendered less efficient, especially those functions demanding mental activity of the higher levels. This decrement in efficiency is due to a breaking down of the powers of inhibition demonstrating itself in a lack of concentration and an inability to fix attention for a period.

3. Speed rather than accuracy suffers.

4. Companionship with the possible factor of competition appears to exert an influence for greater sustained effort in concentration.

5. The effects of fatigue induced through the loss of sleep are relatively immediate, the amount of sleep needed for recovery being dependent upon the individual and usually little or no more than that taken on ordinary occasions.

6. Fatigue itself does not lessen ability but serves to affect those factors of motivation, interest, initiative, attention and concentration, which are necessary to the learning process.

7. Increased individual variability and instability resultant would then seem to be the most outstanding characteristic.

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RELATION OF BODILY TENSION TO ELECTRICAL RESISTANCE[1]

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Problem

The general problem of this experiment was to compare the electrical resistance of the subject during relaxation with his resistance during bodily tension. The electrical resistance of the human body has been studied by many investigators under various conditions. It has been studied with reference to the activity of the sweat glands by Darrow (1), Einthoven and Roos (3), Georgi (4), Markbreiter (6), Waller (11), and Wells and Forbes (14). It has been studied with reference to circulation by Jung (5), Sidis and Nelson (9), and Waller (11). Ebbecke (2), Feri (5), Jung (5), Darrow (1), Priedeaux (10), Sidis and Kalmus (8), Tarchanoff (5), and Veraguth (5) have studied it with reference to mechanical, chemical, and electrical stimulations. No one, so far as the writer has been able to ascertain, has made any thoroughgoing investigation of the relation existing between bodily tension and electrical resistance. This relationship should not be neglected, for if it is discovered that the electrical resistance does vary with bodily tension, it will be necessary to take this fact into consideration in performing experiments with the galvanic reflex and in interpreting the experiments hitherto performed.

Apparatus

The electrodes used in this experiment were made of zinc, and were about two centimeters long and a centimeter wide. These electrodes were covered with kaolin paste and an extremely weak solution of zinc sulphate. In the various series the electrodes were attached to different portions of

[1] From the Psychological Laboratory of the University of Chicago. I am indebted to Dr. C. W. Darrow for aid in this experiment.

the hands. The electrodes were connected directly to Leads I and II of Hindle's Electrocardiograph.

The regular hook up of the electrocardiograph was used with two modifications: Lead III was never used, and the Jacquet Chronograph was substituted as a time marker for the tuning fork supplied with the instrument. The photographic attachment was used throughout.

Procedure

The subject lay on a couch with either one or both hands attached to two electrodes. For relaxation the subject was asked simply to rest and if possible to sleep. The latter did not occur with most of the subjects because there were only two relaxation periods of ten minutes each in an experimental period of forty minutes. For tension, the subject was asked (1) to keep all the muscles of his body as tense as possible for ten minutes, (2) to squeeze a dynamometer as tightly as possible for ten minutes, or (3) to work some multiplication problems mentally. Under the first condition (hereafter called Series I) one electrode was attached to the palm of each hand. Under the second and third conditions (hereafter called Series II and III, respectively) the electrodes were attached to one hand. If the subject were right-handed, the electrodes were attached to the left hand, and vice versa. The experiment was conducted as follows: The electrodes were attached to the hands. The lights were turned out. The subject was told to relax for ten minutes. At the end of the ten minute period he was asked to do either (1), (2), or (3) listed above. Then he was asked to relax for ten minutes more; and for a final ten minutes he was asked to repeat the second condition.

At the start of each experimental period, the tension of the string in the electrocardiograph was adjusted so as to give exactly one centimeter deflection for every millivolt of current. In this way it is possible to compare the deflection caused by the subject with the standard amount and to determine exactly the resistance of the subject by the use of Ohm's law. Ohm's law states:

$\frac{E}{I \times R}$ = a constant, in the equation,

$$\frac{E}{I(R + r)} = \frac{Es}{Is(Rs + rs)}.$$

Es = the standardizing E.M.F.,
Rs = the standardizing resistance,
Is = the current (deflection) with the standard R and E (the deflection is proportional to the current passing through the string and the deflection in centimeters is in each case substituted for I),[1]
rs = the resistance of the string,
E = the potential under the conditions of measurement,
R = the resistance under the conditions of measurement,
I = the deflection under the conditions of measurement. In our problem we have the following values:
Es = one millivolt,
Rs = 40,000 ohms,
rs = 3,200 ohms,
E = one millivolt,
I = one centimeter.

This leaves one unknown quantity R which can be readily calculated.

In each series, the states of relaxation and tension are alternated; that is, each period of relaxation is followed by a period of tension and vice versa. This procedure was followed in order to eliminate to some extent the effect of polarization.

Three times a minute one millivolt of current was shot into the circuit. The deflections caused by the millivolt were measured. This measure was then inserted in the equation and the resistance of the subject then computed. Since the millivolt was introduced into the circuit three times a minute,

[1] See Darrow, C. W., "Sensory, Secretory and Electrical Changes in the Skin Following Bodily Excitation," JOURNAL OF EXPERIMENTAL PSYCHOLOGY, X, 3, June, 1927.

on the average thirty measurements were obtained for each subject during each period. Each subject was used only for one forty minute period.

Series I

The electrodes were attached to the palm of each hand in this series. For tension the subject was asked to contract and hold as rigid as possible all the voluntary muscles, especially those of the legs and arms, but to take care not to squeeze the electrodes. The tension period was of ten minute duration and it was followed by a ten minute relaxation period. For each subject two relaxation and two tension periods were obtained.

TABLE I*a*

Series I

Electrodes Attached to Both Hands

1 Subj.	2 Relax	3 S.D.	4 Tens	5 S.D.	6 D.A.	7 S.D.A.
I	47,181	8,766	28,943	4,485	18,138	1,652
II	57,400	5,439	26,250	0,000	31,150	992
III	28,433	4,362	39,635	506	−11,202	802
IV	64,819	6,551	38,111	661	26,708	1,201
V	42,790	5,834	21,058	1,337	21,638	1,095
VI	41,721	1,949	17,778	1,206	23,193	419
VII	72,785	3,244	29,139	2,565	43,546	754
VIII	93,371	5,350	21,985	2,430	72,386	453
IX	98,100	5,867	77,600	3,459	20,500	1,244
X	36,368	2,772	33,921	1,983	2,347	616
XI	34,631	4,183	19,150	137	15,481	763
XII	53,475	5,695	40,970	2,709	15,505	5,875
XIII	23,792	1,320	23,170	0,000	622	240
XIV	64,612	3,243	14,800	0,000	49,812	592

TABLE I*b*

Series I

Electrodes Attached to Both Hands

Subj.	8 Relax	9 S.D.	10 Tens	11 S.D.	12 D.A.	13 S.D.A.
I	68,740	6,697	36,164	1,174	26,576	246
II	53,613	5,290	35,606	3,232	18,013	325
III	†	0,000	24,760	689	*3,673	113
IV	68,910	5,999	†	†	†	†
V	†	†	†	†	†	†
VI	52,050	0,000	22,992	3,694	30,158	213
VII	67,100	7,125	56,750	798	10,350	4,171
VIII	95,200	6,813	23,406	998	71,794	4,299
IX	104,000	0,000	92,266	2,346	11,734	428
X	37,300	1,559	29,739	2,080	7,561	453
XI	23,170	0,000	21,985	530	1,185	957
XII	46,025	2,695	32,892	1,777	13,133	589
XIII	29,000	1,221	30,085	2,141	1,085	450
XIV	42,587	4,758	24,315	9,480	18,275	613

* Difference of average between 2 and 10.

† Values were not obtained due to some defect in apparatus.

In Tables I*a* and I*b* the averages and standard deviations of fourteen subjects for each period are given. The standard deviations are correct only to the third place. Since a difference of a single ohm is of no significance, it was believed to be useless to use any finer measure in obtaining the standard deviation. In the first column the subjects are indicated by Roman numerals. In the second column is the average of the ohmic resistance during the first period of relaxation; in the third column is the standard deviation of this average; in the fourth column is the average of the period of tension followed by the standard deviation. The sixth column contains the difference between the average in relaxation and the average in tension (D.A.). The standard error is placed in the seventh column as an indication of the reliability of the difference. Table I*b* represents in the same way the data obtained during the third and fourth ten-minute periods of experimentation.

While each of the averages given in the table is based upon at least 30 measurements we are not interested in the particular amounts of resistance as much as in the fact that the resistance in relaxation is different from the resistance in tension. To bring this out more clearly Table I*c* is given.

TABLE I*c*

SUMMARY OF TABLES I*a* AND I*b*

Subj.	Columns 2, 4	Columns 8, 10	Subj.	Columns 2, 4	Columns 8, 10
I	G	G	VIII	G	G
II	G	G	IX	G	g
III	L	G	X	g	G
IV	G		XI	G	g
V	G		XII	g	G
VI	G	G	XIII	g	g
VII	G	g	XIV	G	G

G indicates that the resistance is greater during relaxation than during tension and that the difference is thoroughly reliable.

g indicates that the resistance is greater during relaxation than during tension.

L indicates that the resistance is less during relaxation than during tension and that the difference is thoroughly reliable.

In Table I*c* 'g' indicates that the electrical resistance is greater during relaxation than during tension; 'l' indicates that the resistance is lower during relaxation. The capital

case signifies that the difference is thoroughly reliable; that is, the difference is at least four times the standard deviation of the difference.

Summarizing the table, we find that out of 26 ten minute periods:

(*a*) The average electrical resistance is greater during relaxation in 25 periods.

(*b*) The difference in 18 periods is thoroughly reliable. We may conclude therefore that there is a marked tendency for electrical resistance to be greater during general relaxation than during general contracture.

Series II

In this series the subject squeezed the dynamometer with his free hand during the tension period. He was asked to squeeze it as hard as he could, and yet not to such an extent that he would be unable to hold it for ten minutes. Of course there were minor fluctuations in the intensity with which the dynamometer was held; in fact such fluctuations may be responsible for the slight discrepancy obtained in some of our results.

In this series we had eleven subjects, the results from each of which are given in Tables II*a* and II*b*. The method of obtaining the average resistance for the two periods is the same as the method used in Series I. Our results, however, are slightly different from those obtained in the former series.

TABLE II*a*

Series 2

Electrodes Attached to One Hand

1 Subj.	2 Relax	3 S.D.	4 Tens	5 S.D.	6 D.A.	7 S.D.A.
I	26,140	396	25,876	1,216	264	7,021
II	63,216	4,325	52,050	0,000	11,166	249
III	80,533	9,089	55,400	3,493	35,133	1,776
IV	8,797	634	12,614	2,580	−3,817	483
V	11,186	1,523	18,406	3,728	−7,220	755
VI	39,664	964	27,611	1,180	2,053	278
VII	44,800	1,074	31,563	208	13,237	388
VIII	37,120	2,037	30,711	344	6,409	372
IX	34,435	7,712	14,438	7,871	19,977	2,011
X	80,533	1,777	82,800	1,610	−2,267	438
XI	25,070	498	17,025	5,244	8,045	974

TABLE II*b*

SERIES 2

Electrodes Attached to One Hand

Subj.	8 Relax	9 S.D.	10 Tens	11 S.D.	12 D.A.	13 S.D.A.
I	56,237	4,603	52,050	0,000	4,187	84
II	95,000	3,555	68,800	0,000	26,250	649
III	64,612	4,039	52,050	0,000	12,562	737
IV	13,410	0,000	12,770	5,244	640	956
V	†	†	†	†	†	†
VI	52,050	0,000	49,640	435	2,410	79
VII	23,339	3,168	24,260	3,762	1,921	896
VIII	54,400	7,420	36,933	168	27,467	1,356
IX	21,985	3,251	†	†	*7,547	1,874
X	104,000	0,000	86,400	596	17,600	130
XI	36,400	7,712	23,940	3,168	12,461	1,521

* Difference between averages of item in Column 4, Table II*a*, and Column 8, Table II*b*.

† No values obtained due to some defect in apparatus.

With one or two exceptions, the resistance is never as great as that obtained in the first series. This is, no doubt, due to the way in which the electrodes were attached. The difference between the resistance in relaxation and in tension is not as great in this series as in the other. Probably this result can be explained by the fact that in this series the subject was asked simply to squeeze the dynamometer or to contract primarily the muscles of one arm, whereas in the former series he was asked to contract and hold contracted as many muscles as possible. Table II*c* gives a summary of the results of Tables II*a* and II*b*.

TABLE II*c*

SUMMARY OF TABLES II*a* AND II*b*

Subj.	Columns 2, 4	Columns 8, 10	Subj.	Columns 2, 4	Columns 8, 10
I	g	G	VII	G	g
II	G	G	VIII	G	G
III	G	G	IX	G	G
IV	L	g	X	L	G
V	L		XI	G	G
VI	G	G			

G indicates that the resistance of the relaxation state is greater than the resistance of the tension state and that the difference is thoroughly reliable.

g indicates that the resistance of the relaxation state is greater than the resistance of the tension state.

L indicates that the resistance of the relaxation state is less than the resistance of the tension state and that the difference is thoroughly reliable.

From these tables we note that out of 21 pairs of averages:

(*a*) 18 show that the electrical resistance is greater during relaxation than during tension.

(*b*) 15 out of 18 pairs of averages give differences that are at least four times as great as the standard deviation, and hence are to be considered thoroughly reliable.

(*c*) 3 pairs of averages indicate that the resistance is greater during tension than during relaxation.

(*d*) The differences between average resistance during relaxation and average resistance during tension, for those pairs in which the former are greater than the latter, are more than four times as great as the standard deviation, and hence may be regarded as reliable.

Series 3

This series involved silent multiplication as one of the conditions. The object was to see how electrical resistance during so-called mental work differed from that during so-called muscular work. It is entirely out of the question to define here 'mental' and 'muscular' work; but we do wish to point out some of their similarities and differences in connection with our specific problem. Both of them called for the exercise of the voluntary muscles. Perhaps in the 'mental' task the locus of the muscular activity differed from the locus of the activity in the 'muscular' work. Both the mental and muscular work called for an exercise of volitional judgment. The subject must will to do both. The problem of 'muscular' work, however, was largely to maintain the status quo. It was a spastic affair. The subject endeavored to keep the muscles rigidly contracted either in the body as a whole or in a particular segment of the body. The task we used to represent 'mental' work called for a changing bodily state; that is, the individual must perform several operations to accomplish the task. It was not static.

The subject in this series was given two place figures to multiply by each other during the alternate ten minutes of an experimental period of forty minutes. The subject multiplied silently and then gave an oral answer. As soon as he

gave an answer to one problem he was given another. No attempt was made to check the correctness of the answer given. The results are given in Tables III*a* and III*b*, and summarized in Table III*c*.

TABLE III*a*

SERIES 3

Electrodes Attached to One Hand

Mental Multiplication

1 Subj.	2 Relax	3 S.D.	4 Tens	5 S.D.	6 D.A.	7 S.D.A.
I	86,400	7,871	68,800	0,000	17,600	1,436
II	83,400	7,871	61,600	5,577	24,800	175
III	57,636	3,626	21,016	1,394	36,617	2,237
IV	74,950	9,682	33,567	5,019	41,383	1,787
V	68,800	0,000	80,533	7,417	−11,733	1,353
VI	104,000	0,000	26,473	3,737	77,527	682
VII	92,866	7,417	64,722	5,452	28,144	1,353

TABLE III*b*

SERIES 3

Electrodes Attached to One Hand

Mental Multiplication

Subj.	8 Relax	9 S.D.	10 Tens	11 S.D.	12 D.A.	13 S.D.A.
I	57,633	3,626	21,016	1,394	36,617	708
II	104,000	0,000	80,533	4,326	23,470	3,103
III	63,216	3,614	†	†	42,220*	1,768
IV	46,025	2,695	†	†	12,458*	491
V	92,233	4,413	77,600	6,816	14,433	1,481
VI	68,800	0,000	†	†	42,327*	0,000
VII	104,000	0,000	68,800	0,000	35,200	0,000

* Difference in average between item in Column 4, Table III*a*, and Column 8, Table III*b*.

† No values obtained due to some defect in apparatus.

TABLE III*c*

SUMMARY OF TABLES III*a* AND III*b*

Subjects	Columns 2, 4	Columns 8, 10
I	G	G
II	G	G
III	G	G
IV	G	G
V	L	G
VI	G	G
VII	G	G

G indicates that the resistance of the relaxation state is greater than the resistance of the tension state and that this difference is thoroughly reliable.

L indicates that the resistance of the relaxation state is less than the resistance of the tension state and that this difference is thoroughly reliable.

In 14 pairs of averages from seven subjects there is only one that indicates that the electrical resistance is greater during mental multiplication than during general relaxation. The differences in averages of all the instances are thoroughly reliable; that is, the difference is four times the standard deviation.

Tables III*a* and III*b* with reference to Tables I*a*, I*b*, and II*a* and II*b* suggest that 'mental' work causes a greater decrease in electrical resistance than general or local contracture. A valid difference, however, cannot be thoroughly established from our experiments because the subjects were not the same, and the electrodes were attached in different places.

Conclusion

In general it has been found that there is a higher electrical resistance in relaxation than in tension, and that general contracture differs more from general relaxation than does local contracture. These facts are supported by a comparison of general muscular relaxation (1) with general muscular contraction, (2) with localized muscular contraction, and (3) with bodily conditions during silent multiplication.

The importance of a study of this sort is seen when one notes the numerous experimenters who have measured the galvanic response in terms of ohmic resistance. To illustrate, we may consider Smith's (10) investigation. He balanced the subject's resistance in a wheatstone bridge. As soon as the resistance of the subject changed, the bridge would be thrown out of balance and the galvanic string would deflect. It is conceivable that the deflection of the string, which often has been called the galvanic response, may be due to a change of bodily resistance, due to various organic changes. Moreover such effect may be produced not by particular stimuli, but simply by the situation in general.

The same criticism can be applied to a large number of experiments: Feri, Jung, Sidis and Kalmus, Veraguth, and Waller. Of course, it may be that the state of relaxation did not vary in their experiments; but they at least took no cognizance of it.

By an elaboration of the method, it may be possible to get a measure of relaxation or sleep. It is possible too that this method will throw some light on the relation of the so-called mental states to so-called muscular states.

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THE SPATIAL CHARACTER OF HIGH AND LOW TONES

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In all sensory phenomena one may detect some degree of spatial orientation. The different modalities do not, however, have equal shares of spatial material. The sense of hearing has come off rather badly in this respect. Sounds, in spite of their volumic proportions, are not spread out in space, and localization of them frequently reveals curious inaccuracies and vagaries. And yet for the differentiation of certain tonal qualities it becomes necessary to draw upon terms which are of purely spatial origin. The tones at one end of the pitch-continuum are called high, and those at the other end are called low, and qualitative changes along this continuum are spoken of as rising or falling inflections, ascending or descending cadences, downward or upward movements. The conspicuous fitness of such phrases is immediately apparent.

The application to a given sense impression, of adjectives which belong strictly to impressions from other modalities, occasions as a rule little comment. Colors are called warm and odors heavy not because the psychological thermometers and balances are raised or tipped, but rather because by fairly obvious associations they remind one of experiences in other sense departments. It has been suggested that these cross-modality references give evidence that the varieties of sensory experience have their common origin in an undifferentiated matrix of preperceptual stuff, but it is probably wiser in our present state of knowledge to look upon them simply as evidence of perceived resemblances, or, in some cases, as instances of actual fusions of qualities from different sense departments, as when one calls a flavor puckery.[1] It

[1] Köhler has attached great weight to these cross-modality references in his attempt to account for the facility with which we ascribe to others experiences similar to our own. *Cf.* his *Gestalt Psychology*, 1928, 241 *ff.*

has not been so simple a matter, however, to discover the associative bond which has led so uniformly to the application of the terms high and low to tonal pitch.

Stumpf has found that adjectives meaning high and low (or words closely related in meaning) have been applied to tones in almost every known language.[2] But why should tones be characterized as high or low? Do these characteristics refer to differences in spatial height and depth? The answer to this second question has been almost without exception in the negative. A high tone does not mean a tone which is high in space. The phrase is merely figurative, and must be accounted for in terms of secondary criteria such as, *e.g.*, the apparent localization of high vocal tones in the head and low ones in the chest. The composer Berlioz makes sport of such explanations and reminds his readers that high and low tones for the pianist lie in the horizontal directions of right and left and that the violoncellist must reach downward to produce high tones, and suggests that those composers of opera who use descending passages for a person falling downstairs have stupidly transferred to the tones the arbitrary downward character of the printed notes on the staff. And yet Stumpf, convinced that there can be no intrinsic height and depth in tones, has felt obliged to argue that here again some associative mechanism, strangely obscure and elusive, has been at work. Even Wundt was forced to agree with Stumpf in calling these terms metaphorical when applied to tones,[3] and most psychologists who have given the matter any thought have expressed similar views.[4]

[2] C. Stumpf, *Tonpsychologie*, 1883, I, 192 *ff.*

[3] "Nun sind natürlich die Bezeichnungen 'tief' und 'hoch' für verschiedene Tonqualitäten bildliche Bezeichnungen, die jedenfalls zu einem wesentlichen Teile durch die Gefühle mitbestimmt werden, die sich schon mit den einfachen Tonempfindungen verbinden." W. Wundt, *Grundzüge der physiologischen Psychologie*, 1910, II, 78.

[4] The emphatic naïveté of the physicist, when dealing with matters psychological, is well illustrated by the following quotation. "There is no reason, either physical or psychological, for regarding notes of different pitch as being one 'higher' and the other 'lower.' Considered from the point of view of the pulsation alone, there is no reason for regarding a^1 as being 'higher' than a. The pulsation of the former note is, indeed, shorter by half than that of the latter. But this fact offers no possible excuse for calling it a 'higher' note. . . . Custom should not blind us to the fact that there

What are the associative bonds or the relations of similarity which cause tones to be so appropriately designated as high and low? Stumpf has put himself to great pains to discover possible clues. Since there are no verbal expressions, aside from letters of the alphabet, for tonal qualities, language has borrowed from various sense departments words which apply to impressions accompanied by feelings similar to those to which tones give rise.[5] Thus the affective character of low tones is gloomy and dark while that of high tones is sharp and bright, even painful in very high tones, as though the ear were pierced by a needle.[6] Low tones give the impression of voluminousness and massiveness as contrasted with the thinness and smallness of high tones.[7] The times for *Anklingen* and *Abklingen* of low tones are longer than for high tones, with the result that rapid passages in the upper part of the scale seem light and airy while passages at the same tempo in the bass sound heavy, clumsy, and labored.[8] Even these few examples, among others which Stumpf cites,[9] enable one to discern the associative trail which language has followed in the selection of spatial metaphors for pitch-differences. Thin, small, light, airy: these are terms suitable for objects which, if not always found at high altitudes, are at any rate up and away from the ground. Dark and gloomy objects tend to be nearer the surface of the earth, the massive parts of a structure support the smaller parts, and heavy objects are generally lower in space than light ones.

Such was the type of explanation which Stumpf was forced to adopt in his attempt to account for the use of the words high and low for tonal quality, and most psychologists have been inclined to accept his view.[10] Titchener, *e.g.*,

is no reason other than custom for speaking of one note as being 'higher' than another. Nothing actually happening, either in the air or within consciousness, furnishes any fustification whatever for the practice." J. Redfield, *Music: a Science and an Art*, j1928, 42 *ff.*

[5] Stumpf, *op. cit.*, 202 *f.*

[6] *Ibid.*, 203.

[7] *Ibid.*, 207 *ff.*

[8] *Ibid.*, 211 *f.*

[9] *Ibid.*, 202–226.

[10] Hugo Riemann, after raising objections to most of Stumpf's views on music, seems even to reject Stumpf's conclusion regarding pitch. "Wir verbinden erstens

refers to Stumpf's discussion of the question, and then warns the reader that in the study of tonal sensation he must not be misled by the frequent use of metaphor and analogy; that it is not altogether clear how the adjectives high and low have come to be applied to tones, but that it is quite clear that they must not be taken as indicative of a spatial character in pitch.[11]

To account for cross-modality references by means of association appears, in many cases, plausible enough. If association yields eventually to a more satisfactory principle of explanation it will not be difficult to understand why association was tried. But the most ingenious arguments from association, even in the hands of Stumpf, fail to carry conviction when applied to tonal height and depth. Some of this doubt finds expression in the reservations which Titchener makes when he says that "it is not altogether easy to see" [12] and "we cannot yet say certainly" [13] how the terms high and low came to be applied to tones. Even in the face of this doubt it is legitimate, of course, to suppose that the associative items which Stumpf and others have mentioned serve in some way to reinforce those tonal properties which lead to spatial characterizations. The doubt attaches rather to explanations couched solely in terms of such items. Does not the universality of spatial characterization point to some factor more fundamental than any which derive from cross-modality analogy and supplementation? The experiments about to be reported suggest that the most obvious explanation, but

ganz bestimmt doch auch mit dem einzelnen Tone die Vorstellung oder Empfindung von dessen Stellung in dem keineswegs unendlichen Tonraume und zwar um so bestimmter, je weniger ihm etwas von der Begrenztheit des ihm hervorbringenden Organs anhörbar ist; und zweitens ist die Vorstellung der räumlichen Entfernung zweier einander folgenden Töne eine sehr genau bestimmte, wenn auch nicht nach Centimetern oder Metern, so doch nach Oktaven, Quinten, usw. gemessene. Gerade die offen zu Tage liegende Unterscheidung von Zeitmessung (Metrum, Rhythmus, Takt, Tempo) und Messung der Tonhöhen-Abstände in der Melodiebewegung und im akkordischen Lagenwesen ist ganz besonders geeignet, die Immanenz räumlicher Verstellungen im Tonhöhenbewusstsein zu erweisen." H. Riemann, *Die Elemente der musikalischen Aesthetik*, 1900, 38.

[11] E. B. Titchener, *A Text-book of Psychology*, 1910, 94 *f.*

[12] *Ibid.*, 94.

[13] *A Beginner's Psychology*, 1915, 52.

the one universally rejected,[14] is very likely the correct one, *viz.*, that prior to any associative addition there exists in every tone an intrinsic spatial character which leads directly to the recognition of differences in height and depth along the pitch-continuum.[15]

Observers were asked to locate on a numbered scale running from the floor to the ceiling the position of tones coming from a Western Electric No. 2-A Audiometer. The scale was $2\frac{1}{2}$ meters in height and divided into 14 equal parts. The observer sat facing the scale at a distance of 3 meters, while the experimenter operated the audiometer in back of a large screen to which the scale was attached. Five tones were used: 256, 512, 1024, 2048, and 4096. They were led to a telephone receiver and were presented in haphazard order at five different positions in back of the vertical scale. The observers were allowed to know that the receiver was being placed at different points up and down the vertical scale in order that they might be put on their guard against making judgments on the basis of difference of pitch which

[14] In a recent book by D. W. Prall, *Æsthetic Judgment*, 1929, the suggestion is made that high and low tones may be spatially high and low. "The words we use for these extremes (of pitch) are high and low, but we must remember that we use them somewhat figuratively, as ultimately we use all words. Height is spatial and so is depth, and it is somewhat of an accident, perhaps, that high and low are our most nearly literal description of variations of pitch. . . . And perhaps it is also true that *all high sounds seem to come from higher spatial regions than low ones.* Thunder itself, though it originates in the clouds, comes to us from the trembling earth beneath and all about us, and it is not entirely figurative to say that sounds from heights are high and sounds from depths are low." (83 *f.* Italics are mine.) Just what position Prall is trying to defend here is difficult to say. In the italicized passages he expresses a view which has been generally rejected, but which the present article will attempt to defend. A few sentences earlier, however, he states that the words high and low are used figuratively or accidentally. It is a convenient hypothesis that works equally well in opposing directions! And surely no one will deny that sounds which come from heights are high ones. But how about their pitch? Many sounds, including thunder, have their pitch-salient so obscured that localization of their source (in the clouds or the trembling earth) would reveal nothing about the localization of their pitch.

[15] The idea for the present experiments was first suggested by my colleague, Dr. J. G. Beebe-Center, during a conversation in which the question arose as to whether the apparent movement in a musical phrase might not be due to actual differences in the spatial position of the pitches.

had nothing to do with difference of pitch-location.[16] The instructions to the observers were simple. They were merely asked to indicate by one of the numbers on the scale the region from which the tone seemed to come.

Only at the outset did the observers experience any difficulty with the judgment. The tonal impression seemed at first to pervade the whole room, but as the attentional direction fell in line with the task imposed by the instructions this difficulty entirely vanished and the judgments were made easily and quickly, and with surprising consistency.

The results are clear-cut and unequivocal. *High tones are phenomenologically higher in space than low ones.* For every observer the tones were uniformly placed in the order, from top to bottom, 4096, 2048, 1024, 512, and 256. Not a single inversion of this order occurred in the average values (Table I), and only an occasional reversal appeared between the single items of a series. Hence one may say that of two tones of different pitch the one of greater frequency is called higher, not because of any extraneous associations with altitude, but simply because it is perceived as occupying a higher position in phenomenological space.

TABLE I

POSITIONS ON A VERTICAL SCALE (NUMBERED 1 TO 15) AT WHICH TONES OF DIFFERENT PITCH WERE LOCALIZED

Each figure represents the average of ten judgments

Pitch	A	B	C	D	E	F
4096	12.4	10.4	13.6	13.4	10.0	14.4
2048	9.4	9.4	10.7	11.0	9.1	11.8
1024	7.8	8.3	8.8	8.0	8.4	9.7
512	6.4	7.1	7.2	7.4	6.8	6.9
256	4.6	6.2	6.4	5.4	5.8	1.9

An objection is likely to be raised at this point. May it not be that the observers were not really making judgments

[16] The fact that accurate localization of the source of a sound in the median plane is almost impossible was ignored on the assumption that even if the observers happened to remember this fact they would probably not consider it critically in connection with their instructions, and that they would therefore be set rigidly to judge in terms of pitch-*localization* rather than pitch-*quality*. For three of the observers localization tended to vary directly (but very slightly) with the position of the receiver. For the other three observers the position of the receiver had no effect whatsoever.

of spatial location, but were merely assigning to clearly recognized differences in pitch the spatial characters which inevitably attach to the terms universally employed to designate these pitch-differences? In other words, an observer calls a tone high in space simply because he recognizes it as high in pitch. I can only reply that on the basis of the introspective conviction of the observers and myself I think emphatically that such is not the case. As soon as one has his attention directed to the spatial property of a tone the phenomenon of pitch-locality becomes very real and unmistakable. Moreover, the pitches used in the experiment all bore the octave-relation to each other. Now it is a well-known fact that confusions of the octave within which a pitch lies are very frequent—a judgment sometimes being off by as much as two or three octaves. One would then naturally suppose that if the observers were placing the notes on the basis of pitch-quality the error of octave-confusions (especially since these tones were practically pure) would introduce frequent reversals of localization. But the significant fact is that such reversals rarely occurred. A given pitch was placed always very close to the same point.

These results possess obvious implications and consequences. If they receive further verification, auditory theory must look for the physiological correlate which underlies the spatial difference in pitch. The fact that on any place-theory of hearing the lowest tones would fall at the apex and the highest tones at the base of the cochlea opposite the oval window no more means that we hear the world upside down than the inversion of the retinal image forces us to stand on our heads to see the world right side up.[17] The experiments were done, however, not so much with auditory theory in mind as with the query as to whether the results would throw any light on the moot question of the apparent auditory movement which is set up by tones of different pitch when presented in succession.

We usually think of movement as the change in spatial location of an object which during the change is recognized

[17] One observer stood on his head and found that the direction of high and low tones then seemed reversed.

as the same object or quality. But in musical movement it appeared as though the qualities changed while the spatial location was constant. From the present results, however, one would have to argue that musical movement resembles any other kind of movement to the extent that when different pitches are presented successively they change their spatial location with respect to one another. In presenting successively, *e.g.*, the notes of the diatonic scale from c^3 to c^4 one is aware of upward movement because each note is actually perceived as occupying a higher spatial position than the preceding one. This fact will surely be of importance in the analysis of the variegated movements produced by temporal shifts of tonal quality in music.

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A NOTE ON AN ATTEMPT AT JUDGING OCULAR DOMINANCE FROM PHOTOGRAPHS

BY JUNE E. DOWNEY

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The determination of ocular dominance by various methods has proceeded rapidly since Parson revived the notion that the sighting eye might determine handedness. We have excellent evidence today that about 65 percent of human kind are right-eyed, 30 percent left-eyed, 5 percent impartial-eyed.[1] The dominant eye not only manifests itself in situations that involve aiming, but is undoubtedly a preferred sense-organ which determines many interesting behavior patterns. Monocular vision is a much more common phenomenon than we have been accustomed to believe. During experiments on ocular dominance I have often observed individuals from whose expression I surmised that the non-dominant eye was, for the time being, out of commission—a possibility which raises curious questions concerning suppression of a part of the visual field and suggests for investigation new problems of inhibition.

Probably, also, ocular dominance is a factor in facial asymmetry, another topic of current interest. This topic I propose to discuss from one angle, namely, the possibility of guessing the dominant eye from a photograph. My interest in this possibility was aroused by noticing the extraordinary facial asymmetry evident in such photographs as those reproduced in *Time*, and the apparent ease with which one could determine ocular dominance from them. Of course there was no way of knowing the degree to which such judgments were correct; no doubt they were to a large degree subjectively determined.

Ordinary cuts of every day people appeared less promising

[1] W. Miles, Ocular dominance demonstrated by unconscious sighting, this JOURNAL, 1929, 12, 113–125.

but since no others were at my disposal it was necessary to content myself with this material. There was available a fair supply (1) of photographs reproduced in college annuals, and (2) of photographs taken as part of freshman registration. The first were reproductions of the usual retouched photograph. They were small and lacked uniformity in pose. The second material consisted of untouched photographs, uniform in size and taken from a standardized front position, but not wholly uniform as regards the direction in which the eyes were focussed. For a considerable number of the subjects of these photographs records were on hand for the aiming eye and handedness habits.

The observers in the experiment were five women students in psychology and myself. In a few preliminary tests judgment was passed largely on degree of facial asymmetry, which was determined by reference to such details as relative size of the eyes, drooping of the eyelids, line of regard of the two eyes, collapsing of one side of the face, general expressiveness of the two sides of the face. Later it seemed better to maintain a constant method of judgment. Accordingly each photograph was bisected with a small white card and judged relatively to the comparative expressiveness of the two sides. Ocular dominance was associated with greater expressiveness.

I may first summarize the results of the tests in which I served as an observer. Passing judgment twice on 100 photographs I found that 84 times out of 100 I agreed with myself; 16 times I disagreed. The judgment was therefore fairly constant. Obviously, although the subjective element in such judgments may be great, the judgment is more than a chance guess.

The accuracy of my judgment on 81 cuts in the college annual was 62.9 percent. Of the 81 subjects 46 were on record as right-eyed, 32 as left-eyed, 2 as either-eyed. Out of the 46 possible judgments of right-eyedness, 34 or 73.9 percent were actually given; out of the possible 32 judgments of left-eyedness 15 or 42.8 percent were given. Possibly the figures indicate nothing more than chance success, although, to repeat, the judgment was in no sense a mere guess.

Judgments were also passed on 30 photographs of freshmen girls. In this instance two judgments were given on each, and in case of disagreement three judgments, the record being taken for the two that agreed. 17 judgments, or 56.6 percent, were correct so far as eye-dominance is concerned; 13, or 43.3 percent, wrong. Actual tabulation of the eyedness of the group gave 15 right-eyed, 14 left-eyed, 1 impartial vision. The results obtained, therefore, seem slightly better than chance would lead one to expect.[2]

For testing the observers other than myself I first chose a group of twenty cuts from the college annuals on which it seemed particularly easy to pass judgment. In ten of the photographs there was dominance of the right eye; in ten, dominance of the left eye. Five students in psychology, all women, passed judgment on these photographs so that a total of 100 judgments was given. The actual proportion of right judgments was 76 percent, a percentage high enough to indicate that the photographs were well selected and that choice of them by the author was not motivated by merely subjective factors.[3] In eight out of the twenty cases there was complete agreement among the five judges, and in five other cases there was only one dissentient judgment.

Although the outcome of this series of judgments was satisfactory, further experiments were not encouraging. Two of the group of judges, O and K, who gave judgments on more ample material, were scarcely more successful than chance would dictate. Thus O's success in judging 29 photographs of freshmen girls was only 48.2 percent correct. The 29 photographs were of 19 right-eyed girls and 10 left-eyed. O judged 14 to be right-eyed and 15 to be left-eyed. She has a bias, therefore, toward a judgment of left-eyed.

Of 28 judgments passed by both the author and O on freshmen girls they agreed in the case of 21 of them. This

[2] Miles estimates the reliability of his test for ocular dominance at 95 percent. My records are, I suspect, somewhat less reliable. Inaccuracy in the records would affect the percentage of successes adversely.

[3] Unfortunately in the case of one photograph which all the judges labelled as eft-eyed, it was impossible to check with the original record as this was lost. If thel judgment of all observers is at fault the percentage of correct judgments would be reduced from 76 to 71 percent.

agreement would seem to indicate that something in the photograph determined the judgment.

K passed judgments on 68 photographs in the college annuals with an accuracy of 57.3 percent. Of the photographs 38 were of right-eyed individuals, 30 of left-eyed. K's constant error was an excess of judgments of right-eyed.

Another series was tried using the same five student judges that passed judgment on the group of twenty cited above. In this series 22 cuts of left-handed people were utilized. Ten of these left-handed people were right-eyed, twelve left-eyed. The percentage of correct judgments for the group as a whole was exactly 50! The photographs of left-handed persons are somewhat difficult to handle. Often there is a tilt of the head or twist of the shoulders that baffles an observer.

The above experiment on judging ocular dominance from photographs was obviously not very promising in its outcome. Possibly with better photographs and more rigidly checked records on eye-dominance a higher degree of accuracy could be obtained. Moreover, practice in giving the judgment is probably essential on the part of the observer and possibly a certain amount of special ability.

Summary

Ocular dominance is of interest in connection with the study of facial asymmetry. Success in judging ocular dominance from untouched photographs and from commercial cuts is not pronounced, although the judgment is a fairly constant one. Observers range from 48.2 percent accuracy to 62.9 percent. With photographs selected for the purpose it is possible to obtain an accuracy of 76 percent for a group of observers.

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INFLUENCE OF CHANGING THE EXPECTED TIME OF RECALL

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It was the purpose of the present investigation to study the effect of a change in an expected condition of recall upon the ability to recall. The factor chosen for study was time of recall. The specific problem may be stated as follows. When learning takes place with instruction to recall at a given time, how will a change in the expected time influence the amount recalled?

Expectation with regard to time was established by means of instructions and practice. Each subject learned three lists of paired associates. The first two lists were employed to develop the desired attitude of expectancy and to familiarize the subject with the experimental technique, while a change from the expected condition was introduced with the third list. The procedure may be briefly described as follows:

Group I. Instructions were given for immediate recall. Each of the three lists was recalled immediately following the presentations.

Group II. The subjects were directed to learn for recall on the following day. The first two lists were recalled in accordance with the instructions. The test for recall of the third list was given immediately.

Group III. The directions specified recall on the following day as in Group II. Each list was recalled a day after it was learned and immediately preceding the learning of the new list.

Group IV. As in Group I, instructions were given for immediate recall. For the first two lists the time of recall was identical with the specified time. The test for recall of the third list was not made until the following day.

Groups I and III were the standard groups, with which the experimental groups were compared. Comparisons were made between Groups I and II and between Groups III and IV.

The paired associate method was used. Four lists of twenty pairs of three-letter nouns constituted the learning material. Each subject learned but three of these lists.

The three lists learned and the order of learning them was systematically varied within the group so that all the final group comparisons were based upon all four lists. A Chicago memory drum was employed for the presentation of the words. A stop-watch suspended at the ear served as a timing device. Each pair was exposed for three seconds. The lists were presented three times and recall was tested immediately or on the following day. There were 141 subjects, all students in psychology at the University of Chicago. They were distributed throughout the four groups as to sex and as to graduate or undergraduate standing.

The results are shown in Table I. The first column gives the group number; the second column, the number of subjects in each group; the third column gives the average number of words recalled, and the sigma of the average is found in the fourth column. The difference between the averages compared is shown in the next column, and the sigma of the difference in the last column.

TABLE I

Group	Number	Average	σ Average	Difference	σ Difference
I	32	14.0	.744		
II	32	12.34	.654	1.66	.99
III	38	6.13	.763		
IV	39	5.08	.532	1.05	1.13

When the test for recall is made earlier than expected, the group recalls, on the average, 1.66 words less than when the time of recall coincides with the time expected. When recall is made later than the specified time, the difference between the averages of the standard and experimental groups is 1.05. These differences have a fairly high degree of reliability. When the test for recall is made earlier than the specified time, the difference is greater and more significant than when the test for recall is made later than the expected time, though both changes are detrimental to recall. Thus, it may be concluded that, within the limits of the investigation, when learning takes place with instructions to recall at a given time, a change in the expected time tends to be detrimental to recall.

Several studies relating to expectancy of recall have been made. W. Brown [1] demanded two unexpected recalls from his subjects. As his interest was in the effects of interval and of previous recall, there was no norm with which to compare the unexpected recalls. Thus, there are no data given on effect of unexpectedness.

Boswell and Foster [2] studied the intention of learning for permanent retention as opposed to that for merely temporary recall. Each subject learned on four successive days two series of Chinese-English pairs, one for temporary retention and one for permanent retention. Ten minutes elapsed between the learning of the two lists. Recall was demanded for both series within five minutes and again in two weeks. The total number of right responses in recall after the 5-minute interval was 231 for the temporary series and 219 for the permanent series, and in recall after the 2-week period was 90 for the temporary series and 96 for the permanent series. Boswell and Foster conclude that "the intent to learn for permanent retention really brings about the desired end in the case of learning a vocabulary."

In the series learned for permanent retention, the subjects were instructed that there would be a recall after 5 minutes for the sake of comparison, but that the real test would be made in about two weeks. In the case of the lists learned for temporary retention, the subjects were told only of the immediate recall. Since both recalls were expected in the series for permanent retention while the second recall was unexpected in the series for temporary retention, the comparative result for the delayed recall may indicate the relative effectiveness of an expected or an unexpected recall, but the data throw no light on our problem as to the influence of a change in expected time of recall.

(Manuscript received October 15, 1929)

[1] W. Brown, Effects of interval on recall, this JOURNAL, 1924, 7, 469–475.

[2] F. P. Boswell and W. S. Foster, On memorizing with the intention permanently to retain, *Amer. J. Psychol.*, 1916, 27, 420–426.

CONCERNING THE ALLEGED CORRELATION OF INTELLIGENCE WITH KNEE JERK REFLEX TIME

BY JOHN C. WHITEHORN, HELGE LUNDHOLM AND GEORGE E. GARDNER

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The recent reports of Travis [1] and Travis and Hunter [2] indicating an extraordinarily high coefficient of correlation, in the negative sense, between intelligence and the 'knee jerk reflex time,' led us to undertake a study of this time-measurement in the hope of finding therein a physiological correlate to mental disturbances in psychotic patients.

As a preliminary to the study of our special problems, we undertook a comparison of the 'knee jerk reflex time' in a group of feeble-minded persons as compared with normal persons. Opportunity to make such observations was kindly granted by our good neighbor Dr. Ransom A. Green, superintendent of the Walter E. Fernald State School, Waltham, Mass.

In our experience, the reflex times of the feeble-minded subjects have not been slower than those of normal controls. When we wrote Dr. Travis concerning our results, he informed us that later investigations in his laboratory have also failed to confirm his earlier reports. We wish, therefore, to present our observations with the greatest possible brevity, to the consideration of those who may have become interested in this alleged relationship.

Measurement of Reflex Time

The term 'knee jerk reflex time' we shall use here, as does Travis, to indicate the period of approximately 0.020 second which elapses between a mechanical tap on the ligamentum patellæ and the beginning of the electrical disturbance (in

[1] L. E. Travis, *Science*, 1928, 67, 41.

[2] L. E. Travis and T. A. Hunter, this Journal, 1928, 11, 342.

the extensor muscles) which initiates the muscular contraction responsible for the jerk of the lower leg. This 'end-point' of the reflex time is recorded through an electrode placed on the thigh midway between the anterior superior spine of the ilium and the upper border of the patella. As Travis has pointed out, this electrical disturbance occurs considerably earlier than the actual movement of the lower leg. Our electrical and photographic apparatus for the measurement of this time interval, which are to be described in detail elsewhere,[3] differ considerably from those of Travis, as we preferred to use a string galvanometer without amplifying devices. What difference in result, if any, is to be attributed to these differences in technique, we cannot state, as we have not had the opportunity to make direct comparisons.

Subjects

The subjects of low mental age were from the Walter E. Fernald State School. All the facts concerning this group, except the reflex time, were provided from the records of that institution. We wish to express our grateful thanks for this aid, particularly to Dr. R. A. Green and Dr. C. S. Woodall.

The 'normal controls' were employes of this hospital within the same range of chronological age as the feeble-minded group. One was a medical student, nine were nurses and three were attendants. As a check on the assumption of normal mental ability, these subjects were given the 'Army Group Examination Alpha-Form 9.' All subjects were male.

Results

Ten tests of reflex time were made on each subject (five on each leg). The average values, together with the standard deviations for each subject, are presented in Table I, together with other pertinent information.

This table exhibits clearly the differences between individual persons. But there is no tendency for *low* mental ages to correlate with *slow* reflex times. There is, indeed, a perceptible statistical trend in the opposite direction. Pear-

[3] J. C. Whitehorn and H. Lundholm, *Amer. J. Physiol.*, 1930, 92, 214.

TABLE I

Subject Group	Number	Mental Age Yrs.	Mo.	Chronological Age Yrs.	Mo.	Height Inches	KJ Reflex Time Seconds × 1000 Mean	S.D.
W.E.F.S.S.	I	1	6	18	1	61	18.3	0.30
"	II	1	6	18	11	55	19.6	0.48
"	III	3	0	19	1	68	21.6	0.35
"	IV	3	2	24	3	63.5	19.1	0.24
"	V	3	8	20	4	66.5	20.0	0.25
"	VI	4	4	25	3	67	20.7	0.49
"	VII	5	8	22	10	66	20.9	0.35
"	VIII	7	2	19	2	65.5	21.1	1.05
"	IX	8	0	22	9	63	20.1	0.47
"	X	9	9	18	6	69.1	20.4	0.39
Control	I	10	6	22		66.5	21.9	0.32
W.E.F.S.S.	XI	10	7	17	5	60.5	18.3	0.13
"	XII	10	8	18	1	70.5	23.2	0.32
"	XIII	10	11	19	6	61.8	21.9	0.65
Control	II	12	9	21		66.5	19.9	0.17
"	III	13	6	18		65	20.2	0.61
"	IV	16	3	21		61.8	18.7	0.36
"	V	16	6	24		66.3	22.6	0.32
"	VI	16	6	24		69.8	24.0	0.44
"	VII	16	6	19		66	21.4	0.54
"	VIII	16	6	20		67.3	21.9	0.31
"	IX	16	9	21		68.5	22.8	0.66
"	X	17	3	23		64	20.9	0.40
"	XI	19	6	22		64.3	20.2	0.44
"	XII	20	0	24		70	21.8	0.52
"	XIII	20	3	24		69.8	22.3	0.41

son's coefficient of correlation between the mental age and the knee-jerk reflex time is $+0.37 \pm 0.11$ for the data in Table I. This can be attributed, however, to differences in height, for several of the feeble-minded were poorly developed physically, and short persons have quicker knee jerks. When appropriate allowance is made for the influence of stature, the correlation becomes negligible, $+0.15 \pm 0.13$.

Conclusion

The alleged correlation between mental age and knee jerk reflex time has not been confirmed in this comparison of normal and feeble-minded subjects.

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